THURSDAY, FEBRUARY 14, 1884

## MR. RUSKIN'S BOGIES

PROFESSOR RUSKIN'S utterances are perhaps to be taken least seriously when he is himself most serious, and probably he was never more in earnest than in his jeremiad on modern clouds, delivered at the London Institution on the 4th and 11th inst. Probably none of the readers of NATURE have been terrified by the storm cloud of the nineteenth century, but should it be otherwise we hasten at once to their relief. Twenty years before the date fixed by Mr. Ruskin for the first appearance of his portentous "plague-cloud," the writer of the present article commenced a series of observations on the forms and structures of clouds, followed a few years later by such daily charts of wind and weather as could be constructed from the data, somewhat meagre, that were then accessible. As might be expected, cyclone and anti-cyclone were then as they are now. The dimensions and densities of the cloud layers have not altered, neither has our moral degeneracy nor the increased smoke of our manufacturing towns developed any new form of cloud. Neither (until the phenomenal sunrises and sunsets of the last three months) has Nature, in painting the clouds, employed upon her palette any fresh tints, whatever artists may have done. Further, we have not observed, nor met with any one, except Mr. Ruskin, who has observed, that the wind during the last thirteen years has adopted a "hissing" instead of a "wailing" tone, or that the pressure anemometer indicates that the motion of the air has become more tremulous than heretofore.

Admiration ought ungrudgingly to be bestowed on one who has done good service as an art critic and as a contributor to English literature. The sympathy, moreover, which, denied to those who are in advance of their age, is naturally accorded to the archaic type of mind, is enhanced by the attractiveness of a personality whose idealism is as lofty as that of Mr. Ruskin. But we maintain that there is a further sentiment which contributed to the applause which Mr. Ruskin's audiences bestowed upon him. Speaking generally, "broadly and comfortably," as he would say, Mr. Ruskin is not a representative man, yet he represents a certain spirit of Philistinism (for it merits this name), which is far from being unpopular, and which shows itself in opposition to scientific culture. He is the spokesman of that mental attitude which misinterprets the province of science and affects to misunderstand the plainest utterances of the physicist. "The first business," he says, "of scientific men is to tell you things that happen, as, that if you warm water it will boil." "The second and far more important business is to tell you what you had best do under the circumstances—put the kettle on in time for tea." "But if beyond this safe and beneficial business they ever try and explain anything to you, you may be confident of one of two things—either that they know nothing (to speak of) about it, or that they have only seen one side of it, and not only have not seen, but usually have no mind to see, the other. When, for instance, Prof. Tyndall explains the twisted beds of the Jungfrau to you by intimating that the Matterhorn is growing flat, or the clouds on the lee side of the Matterhorn by the winds rubbing against the windward side of it, you may be pretty sure the scientific people do not know much (to speak of) yet either about the rock beds or the cloud beds. And even if the explanation, so to call it, be sound on one side, windward or lee, you may, as I said, be nearly certain it will not do on the other. Take the very top and centre of scientific interpretation by the greatest of its masters. Newton explained to you—or at least was supposed to have explained—why an apple fell [*sic*], but he never thought of explaining the exactly correlative but infinitely more difficult question how the apple got up there." One would have supposed that even the lecturer must be aware that modern science is at least as much occupied with the last as with the first of these problems. Mr. Ruskin has not yet done with Prof. Tyndall;—in other words, he can nowhere suppress his dislike of scientific thought. "When I try to find anything firm to depend on, I am stopped by the quite frightful inaccuracy of the scientific people's terms, which is the consequence of their always trying to write Latin-English, and so losing the grace of the one and the sense of the other." "I am stopped dead because the scientific people use undulation and vibration as synonyms. 'When,' said Prof. Tyndall, 'we are told that the atoms of the sun vibrate at different rates, and produce waves of different sizes, your experience of water waves will enable you to form a tolerably clear notion of what is meant.' 'Tolerably clear,' your toleration must be considerable then. Do you suppose a water wave is like a harp string? Vibration is the movement of the body in a state of tension, undulation that of a body absolutely lax. In vibration not an atom of the body changes its place in relation to another; in undulation not an atom of the body remains in the same place with regard to another. In vibration every particle of the body ignores gravitation or defies it; in undulation every particle of the body is slavishly submitted to it." And more of the same sort. We should not weary the reader with these quotations were it not too true that much of the poetry which Mr. Ruskin adores, and much of the art of which he is the apostle—not a little in short of the poetry and art of our day—are full of this anti-scientific Philistinism, whose ideal is ever in harsh contrast to the real, and which from its antagonism to the facts of Nature is the great producer of bogies. One has only to go through any picture exhibition to see plenty of those clouds which Mr. Ruskin persuades himself occur in Nature, which, "irrespective of all supervening colours from the sun," are intrinsically "white, brown, grey, or black"; "argent or sable, baptised in white, or hooded in blackness."

We recommend those who sympathise with Mr. Ruskin to study some of those little books which are beginning to be the delight of our children. Such readers may never attain the scientific spirit, yet they may possibly catch a few chords of that great song in which there is complete harmony between the Universe of Nature and that of poetic and artistic sentiment, whose faint beginnings will alone be heard in this plague-stricken century.

Against cloud-classification the stars in their courses have hitherto fought, and Mr. Ruskin in his continues the battle. Grievous are the wounds which he inflicts

Let us see how he heals them. ' Every cloud is primarily definable—' visible vapour of water, floating at a certain height in the air.'" It is thus distinguished from that "form of watery vapour" which "exists just as widely and generally at the bottom of the air as the clouds do on what for convenience' sake we may call the top of it." Mr. Ruskin hopelessly confuses vapour with water-dust, and this confusion leads him into some amusing difficulties. He asks whether it is "with cloud vapour as with most other things, that are seen when they are there, and not seen when they are not there, or has cloud vapour so much of the ghost in it that it can be visible or invisible as it likes, and might, perhaps, be all unpleasantly and malignantly there just as much when they did not see it as when they did?" To this he answers "comfortably and generally" that "on the whole a cloud is where we see it, and not where we do not see it," and that we must not allow the scientific people to tell us that rain is everywhere, but palpable in one place, impalpable in another. He presently returns to his point. He has defined a floating or sky cloud, and defined the falling or earth cloud (which by the way had been altogether excluded by his first definition from his category of clouds). "But there is a sort of thing between the two which needs another sort of definition, namely, mist." The definition of this intermediate substance, however, Mr. Ruskin does not supply, being content with asking what difference there is between clear and muddy vapour. This division of clouds has at least the merit of brevity, although it is subsequently complicated by a further division into "two sorts of clouds, one either stationary or slow in motion, reflecting unresolved light, the other fast-flying and transmitting resolved light. [Really, clouds at a distance and clouds overhead.] As regards the difference in the nature of these, Mr. Ruskin merely "hints to us his suspicion that the prismatic cloud is of finely comminuted water or ice, instead of aqueous vapour";—it is difficult to understand what he supposes the former kind of cloud to be composed of.

During the forty years previous to 1871, according to the certificate of Mr. Ruskin, the clouds, thus divided and cross-divided, appear to have behaved themselves in a peaceable and orderly manner. Even the "thunder-cumulus" (English-Latin, by the way) did "its mighty work in its own hour and in its own dominions, not snatching from you for an instant or defiling with a stain the abiding blue of the transcendent sky, or the fretted silver of its passionless clouds" We may remark that these "good, old-fashioned, healthy storms" frequently had rather extensive dominions: *e.g.* on August 13, 1857, one of these storms was simultaneously felt over many thousand square miles, and extended from the Land's End to John o' Groat's, besides covering a very extensive district on the north-western parts of the European continent. The deportment of the great bogy meteor, "storm-cloud or more accurately plague-cloud," of the nineteenth century is exceedingly different. From one part of Mr. Ruskin's description of this phenomenon we imagined that he might allude to the sheet of stratus commonly occurring in winter anti-cyclones, a sheet which occasionally covers upwards of 60,000 square miles, with scarcely a rift in its surface, the greatest vertical thickness of the cloud being only 300 or 400 feet. But this illusion was soon dispelled. For we find that "in the plague-wind the sun is choked out of the whole of heaven all day long by a cloud which might be a thousand miles square and five miles deep." One would scarcely have expected so dense a cloud mass merely to turn the sun red, but Mr. Ruskin is angry with it for not doing so: "That thin, scraggy, filthy, mangey, miserable cloud, for all the depth of it, could not turn the sun red as a good business-like fog did with a hundred feet or so of itself." Further, it is accompanied by a terrible wind by which "every breath of air is polluted half round the world" [*sic*]. Mr. Ruskin omitted to mention the effects of this plague-wind on agricultural or vital statistics. "It is a wind of darkness," also "a malignant wind." Further, "it always blows tremulously, making the leaves of the trees shudder as if they were all aspens but with a peculiar fitfulness which gives them an expression of anger as well as of fear and distress." Further, "it pollutes as well as intensifies the violence of all natural and necessary storms." Here again some explanation is sorely needed, since we should much like to know whether during the plague-wind barometric gradients become steeper, or whether the force of the wind in relation to the gradient is greater than usual.

Enough for the present of such bogies; although we fear that we have by no means done with them until our literary men will master the simplest elementary primers. But not enough of Mr. Ruskin, whom we could ill spare. His English is often delicious; always in his most dyspeptic diatribes amusing. And we can all appreciate his concluding advice that we should "bring back our own cheerfulness and our own honesty; and cease from the troubling of our own passions," and (not least we think of all) "the insolence of our own lips." A good recipe: add a dash of humility and of respect for the opinions of wiser men;—and all may yet be well, even though our return to the paths of rectitude should fail to dissolve the "mangey" clouds, and quench the fevered wind of a storm-harried and woe-worn era.

W. CLEMENT LEY

## SPINOZA

*Ethic.* By Benedict de Spinoza. Translated from the Latin by William Hale White. (London: Trübner and Co., 1883.)

IF proof were requisite that the standard of value in philosophy is different from that which obtains in the estimation of scientific research, it would only be necessary to point to the case of Spinoza. There is probably no thinker of the nature of whose work there obtain conceptions more hopelessly irreconcilable; there is certainly none about whose position there is more general unanimity. To refer to the more recent of his English critics, Prof. Caird and Mr. Frederick Pollock are at one in assigning to Spinoza most important functions in the development of philosophical inquiry. Yet there is scarcely a single point in his system as to which their respective interpretations are not mutually exclusive. But as regards the broad feature which makes Spinozism deeply interesting to students of science in the strict sense there can be no doubt. The application of the method of geometry to philosophical problems finds its counterpart in the prevailing, and apparently by no means diminishing, disposi-

tion to bring certain questions of metaphysics within the scope of scientific inquiry. That any one should have rejected the current method of metaphysics in favour of a geometrical investigation into the nature of God and existence, cannot be otherwise than significant to persons who seek to determine the psychological problem of the nature of consciousness by physiological means. Hence it is that there are some students who think that, if any philosophy were possible, it were that of Spinoza, and others who say that in the work of Mr. Spencer and Prof. Clifford they find the inheritance which Spinoza left behind him.

Mr. Hale White has done his difficult work well. The translation is executed with great care, and the style of the original has been reproduced with some success. That English readers of Spinoza have entertained very loose notions of his real teaching has been due in no small measure to the very inaccurate translation which has hitherto passed current. The present volume should do much to improve the popular conception of Spinoza's system.

At the risk of repetition of what has already been insisted on in these columns, it is right to contrast the position of the naturalists who accept Spinoza's application of scientific methods to metaphysical questions, with the procedure of Kant and those who are currently described as Neo-Kantians. It is the more desirable to revert to this topic because, although there is much complaint that the Neo-Kantians do little (if anything) more than repeat Kant's criticism of the naturalist (or, as he would have described it, dogmatic) doctrine, there is but little evidence that this criticism has been considered, much less met. People go on reasoning upon the old lines about the relation of mind to body and of God to the world as if Kant (to borrow a phrase from another branch of learning) had never obtained a rule calling upon them to show cause why there should not be a new trial of all such questions. It cannot be sufficiently borne in mind that at the present time there are only two courses open in this reference to conscientious thinkers. Either they must abstain altogether from the discussion of an increasing number of problems which are suggested by scientific inquiry, or they must be at the pains, however irksome, to master the nature of the sceptical doubts which Kant brought to bear upon the possibility of these problems. And it may be added that to single them out for elimination is not so easy a task as might be supposed. Probably the real reason why the study of Spinoza's ethics is attended with so much difficulty is that the extraordinary instinct which guides men of the highest genius in inquiries in new and unknown regions raised doubts in his mind which the investigations of Kant subsequently exhibited as the consequences of a more profoundly sceptical point of view. That difficulties arise when men reflect upon the nature of God was for Spinoza, as for Kant, due to the impossibility of reasoning on such matters as if they were ordinary facts of experience. In Spinozism the geometrical method culminated in the abrupt cessation of thought of this kind, just as in Hume empiricism ended in the paralysis of speculation. Had Spinoza pressed his distinction between different kinds of knowledge further, his system must have become in a greater or less degree sceptical in its tendencies—sceptical in the sense in which Kant was sceptical as a preliminary to reconstruction, or in which, to take the case of a very recent scientific writer, the late Prof. Clifford was a sceptic when he completed his analysis of experience with his theory of ejects. The difference between the three cases is that Kant clearly saw the origin and nature of the difficulties raised by himself, and made the inquiry the preliminary to a radically different discussion of the issues raised in philosophy and science alike. It were well if the fact were less left out of account that the rule obtained by Kant for a new trial of these issues has never yet been discharged. R. B. HALDANE

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## LETTERS TO THE EDITOR

*[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

*[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.]*

### The Krakatoa Eruption

THE Council of the Royal Society has appointed a Committee for the purpose of collecting the various accounts of the volcanic eruption at Krakatoa, and attendant phenomena in such form as shall best provide for their preservation and promote their usefulness.

The Committee invite the communication of authenticated facts respecting the fall of pumice and of dust, the position and extent of floating pumice, the date of exceptional quantities of pumice reaching various shores, observation of unusual disturbances of barometric pressure and of sea-level, the presence of sulphurous vapours, the distances at which the explosions were heard, and exceptional effects of light and colour in the atmosphere.

The Committee will be glad to receive also copies of published papers, articles, and letters bearing upon the subject.

Correspondents are requested to be very particular in giving the date, exact time (stating whether Greenwich or local), and position whence all recorded facts were observed. The greatest practicable precision in all these respects is essential.

All communications are to be addressed to

G. J. SYMONS,
Chairman Krakatoa Committee

Royal Society, Burlington House, W., February 12

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### The Remarkable Sunsets

THE following facts in reference to the unusual sunsets, as witnessed in the United States, will I hope be of sufficient value to your readers to justify an insertion in the pages of NATURE.

The place from which I write is 1063 feet above sea-level, 40° 48′ 47″ N. lat. and 81° 53′ 37″ W. long. from Greenwich. The main features of the exhibition here have been the crimson glow—the first and after-glow, with other accompanying colours, closely corresponding with those in England and Europe. Hence I need not occupy your pages with a special description.

I have on record seven cases, nearly all the weather would permit one to see. These occurred on November 27, December 9, 10, 25, and 28, and on January 13 and 17.

The first and second glow have extended in two or three instances, though faintly, to the zenith, and the first has occasionally been reflected on the eastern sky. On December 28, the most brilliant exhibition in the series, an arch was formed in the east, the colours red and yellowish green, very soft, and much blended. The crimson glow on the sky flooded the western sides of buildings with an unearthly light, and cast faint shadows across the snow. The appearance of the after-glow, when the sun had reached a certain angle in its decline, favours the view that it is a reflection of the first. If this be true, it is not neces-

sary to admit so great an elevation of the reflecting matter above the earth, and thus removes a serious difficulty in explaining the glow by known causes.

In no case here has the sun during the day or at setting appeared green. On December 28 and January 13 Venus has appeared a beautiful green through the complementary crimson. This fact became important only when it was discovered that the green remained after the crimson had disappeared. The light of the planet was struggling through some medium invisible to the eye, but which arrested the other colours.

Another important point. The glow has been seen without the slightest trace of cirrus clouds behind it. Three times faint ribbon-like stripes of cirri appeared in the first glow, but in the second the gorgeous crimson has generally been projected against the clear blue sky.

The writer has seen no notice of observations on the appearance of the sun and sky during the day, and especially the afternoon, before the brilliant sunsets.

The peculiar appearance of the atmosphere in the vicinity of the sun attracted his attention on the day the first remarkable sun glow occurred. The sky was perfectly clear except around the sun, which was embedded in a soft haze that extended out some 6° or 8° on every side. Yet a distant boundary could not be assigned to the haze, so gradually did it shade into the blue of the sky. The sun was obscured so that the eye could look at it for a moment and outline its disk. Covering the sun with the hand the haze adjacent glowed like a furnace, the light diminishing rapidly as the eye swept outwards.

Two or three remarks, naturally spring from this appearance. 1. The haze was not an ordinary cirrus cloud. It had no distinct bounding surfaces; it was invisible everywhere except near the sun. 2. There was, of course, no more of the matter forming the haze around the sun than elsewhere. 3. It was capable of reflecting intensely the light that fell upon it at a large incident angle, nearly 90°. 4. The reflection of light in a high degree by any substance at a large incidence would indicate a liquid. But the clearness of the sky showed the absence of condensed vapour. And yet there was something in the air around the sun—and no more there than anywhere else—which was then, some three hours farther east, flinging its gorgeous crimson over earth and sky, and which, three hours later, would drape the earth and sky of the observer in the same beautiful colours. And what was that something? That is doubtless the great question, and I can only echo, What was it? If the answer be "Vapour of water in some peculiar state," then it is wondrous strange that water, subject as it always has been to almost every conceivable change in the air, should rarely if ever before have assumed this peculiar state! Besides, the prevalence of this phenomenon around the globe, manifesting the same characteristics everywhere, requires some marked and probably unusual cause.

As to the volcanic theory, it has some good points. It gives an unusual explanation for an unusual occurrence.

It might be expected that a convulsion which would ingulf islands and mountains, and send the throbbings of ocean around the globe, would leave some tokens of its presence on the more sensitive air.

The difficulty of accounting for the suspension of solid particles for months in air of extreme rarity may be avoided by admitting the effects to be due mainly to gases ejected in the eruptions. Most of these being condensible by extreme cold would occupy definite strata and not rise to an extreme height.

The sinking of Krakatoa and the admission of sea water to the awful and fiery gulfs below, would, it seems, set free immense quantities of chlorine from the salt water. As this gas is readily absorbed by pure water it may have condensed around its molecules the vapour of the air, and thus become capable of reflecting the light in a higher degree.

Of course these are suppositions, consistent as far as we know with law; and they may stand among other probabilities till clearer light confirms or rejects them.

In a communication to NATURE, December 13, p. 149, Prof. C. Piazzi Smyth advances the idea that one of the conditions of the red sunsets was the *dryness* of the *lower atmosphere*. The hygrometric condition of the air here on the days the crimson sunsets were seen, is given in the following table, taken from the monthly reports of the writer to the U.S. Signal Office. The two columns give the mean temperature of the dry and wet bulb thermometers (F.) for three observations each day, at 7.32 a.m., 2.32 and 9.32 p.m.

| Dates | Dry bulb | Wet bulb |
|---|---|---|
| November 27 | 29° | 26·8° |
| December 5 | 40 | 38 |
| " 9 | 36·6 | 32 |
| " 10 | 36 | 33 |
| " 25 | 29·5 | 26 |
| " 28 | 23·6 | 22·2 |
| January 13 | 37 | 33 |
| " 17 | 23·2 | 21·2 |

A mere inspection of the table shows that the dew point was high, and the percentage of possible moisture in the air quite large. Whether this weird and beautiful play of colours around the dying day is due to watery vapour in the air time will show; here it has certainly not been due to any *deficiency* in the vapour of the lower strata.

O. N. STODDARD

Wooster, State of Ohio, United States, January 18

## Unconscious Bias in Walking

THE following little experiment seems to show that if the majority of people are, as Mr. Darwin argues, left-legged, they would circle to the left in a mist, as Mr. Larden says they do. I would call myself normal, my left leg being the stronger. That is to say, like the majority, I jump from the left, rest my weight standing on the left (a glance at a photograph album shows this to be normal) and generally cross my right over my left whilst sitting. Having put myself in a dark empty room, I could not satisfy myself as to which way I circled, there not being space enough, but when I artificially lamed myself by putting a few tin tacks in my slipper, I circled strongly in the direction of the sound foot. From what had been said in NATURE on the subject at the time, I expected the for-the-time-being longer and stronger limb to circle round the other. The fact seems to be that there is a bias towards the stronger, most-leant-upon limb, irrespective of length. It is worth noting that, if the object causing pain be placed under the inside of, say, the right foot only, the experimenter will lean on the outside of that foot and circle to the right.

In the matter of left-leggedness I have requested reveral right-handed people to feign lameness. Every one of them has limped with the right foot; and, on being asked to do so, has found difficulty in imagining the left lame, and acting as if it were. May it not be because the right leg is somewhat weaker that canes are carried in the right hand?

But although left-leggedness *quâ* strength seems normal, the reverse seems to hold good *quâ* skill: one pushes a door to with the right, feels his way down a dark stair with the right, kicks a football with the right. A friend of mine, a skilful athlete, particularly known as a jumper, at first expressed astonishment that there should be any doubt as to the left leg being the stronger. On reflection he added: "I'm not sure, however; figures in skating are easier on the right." This nine figure-skaters out of ten will assent to. It is to be expected, if my theory is correct. The right leg is more easily controlled, guided, and kept in position—in a word, the more skilful limb; and at the same time the left being the better kicker, the impulse is better given.

It seems to me that mounting a horse from the near side is not a mere fashion (except for the left-handed minority). The stronger leg is put in the stirrup and gives the lift, whilst the more skilful leg is thrown over the animal's back.

It would be interesting to know which foot it is, if any in particular, which Indian servants use for prehensile purposes; also whether the higher quadrumana are right or left hind-handed.

I have noticed that persons walking in the street dwell longer on the one foot than on the other, and I remember once arguing that in-toed persons with a rolling gait were the only people who were not lame. I have been trying to observe this seriously for some days, and believe it to be so, but as the mind naturally invents a beginning and an end for a continuous motion it may be imagination.

W. G. SIMPSON

5, Randolph Cliff, Edinburgh, February 6

## The Ear a Barometer

THE phenomenon described by my friend Mr. Boys, on p. 333, is pathological, and not physiological. He is clearly suffering from slight obstruction of the Eustachian tube, a canal which leads from the inner side of the tympanic cavity into the posterior fauces. Its natural relief is, as he very accurately describes, by

the act of swallowing, which temporarily distends the tube. He can test its perviousness by holding his nose with his fingers and forcing air into the nasal cavity. Physicians are in the habit of placing an ordinary stethoscope over the ear, causing the patient to go through the act of deglutition, and listening for the "click" of escaping air. Mr. Boys will see, as a physicist, that, if the access of air on either side of the tympanum were free, increase or decrease of atmospheric pressure would make no difference.

14, Dean's Yard, February 10 W. H. STONE

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WITH regard to the letter of Mr. C. V. Boys in NATURE of February 7 (p. 333), I should like to make a remark or two on the matter, in which I have had practical experience. I am in the habit of running between Rugby and London daily, and pass through six different tunnels on the route. The Leighton tunnel is divided into three parts, the down fast line being single, and the space between the rails and the walls of the tunnel very small. On entering this, if in the first three coaches next to the engine, a sudden expansion of the tympanum is felt. I have been led to account for this phenomenon as follows: The engine acting as a piston forces the air before it through the tunnel, and so causes a partial vacuum, which extends to the first three or four coaches. After that the air has had time to rush in and fill the empty space, and this explanation is rendered almost certain by the fact that at the end of the train of twelve or fourteen coaches no aural effects are observable, thus demonstrating that the sudden propulsion of the air through the tunnel is compensated for before the middle of the train has entered. In Kilsby tunnel nothing has been noticed by myself. I account for the pressure alteration in the above manner, the engine and the tunnel-mouth closely fitting, and so are fairly comparable to a piston within a cylinder. The effects decrease from the engine to the end of the train, and are practically unobservable in the last few coaches.

Rugby, February 9 GEORGE RAYLEIGH VICARS

## Diffusion of Scientific Memoirs

ALLOW me a few final words on this curious case. I spoke of the *Trans. C. P. S.*, 1849-54, in which Stokes' papers were "buried," as "almost inaccessible." This expression was challenged by the ex-Secretary of the Society, and I replied that the question could be decided by statistics alone. I indicated what statistics were required, and waited some weeks for them. The present Secretary then gave me the less essential part of the desired information, and I proceeded to make the best I could of it. Now I am told that I misunderstood his object, and that he practically admits what his predecessor challenged.

I also stated that my copy of the *Proc.* was very imperfect, and that I had not received any *Trans.* I was then told that "publications" were given only on application. If so, I replied, I should have had all, or none. To this there is no answer.

P. G. TAIT

Coll. Edin., February 9

## Wind Sand Ripples

SOME time ago, whilst reading an account in NATURE of very ingenious and interesting experiments by Prof. G. H. Darwin on sand ripples, my memory was recalled to some very beautiful sand ripples caused by the action of wind, seen by another person and myself on the west coast of Ireland, near Bundoran. The locality was a sand ridge twenty or thirty feet above high-water mark, and beyond the influence of either sea or river action; the ripples extended over a space of twenty yards or more. At the time there was a fresh breeze, with frequent squalls, blowing across this ridge. This ripples moved before the wind at the rate of about a foot in three or four minutes, but faster during the squalls, retaining all the time (I watched them an hour or more) perfect uniformity of shape and size. The distances were roughly measured by sticking up in the sand bits of wood at, as nearly as could be guessed, one foot apart, in a line with the direction of the wind. The ripples were about three inches from summit to summit, and the depth of trough three quarters of an inch.

The time was carefully noted with a watch. The forward movement of the ripples was evidently caused by the sand being drifted from their weather sides, and deposited on their lee, and thus there was a progressive movement to leeward, more or less rapid according to the increase or diminution of the wind force.

4, Addison Gardens, February 9 JOHN RAE

## Animal Intelligence

THE following anecdote, received the other day from Russia, may possibly interest your readers:—"The following was narrated to me by Mohl's brother, on whose estate it took place. The carcass of a cow was laid out in the woods to attract the wolves, and a spring-trap was set. Next morning the forester found there the track of a bear instead of a wolf on the snow: the trap was thrown to some distance. Evidently the bear had put his paw in the trap and had managed to jerk it off. The next night the forester hid himself within shot of the carcass to watch for the bear. The bear came, but first pulled down a stack of firewood cut into seven-foot lengths, selected a piece to his mind, and, taking it up in his arms, walked on his hind legs to the carcass. He then beat about in the snow all round the carcass with the log of wood before he began his meal. The forester put a ball in his head, which I almost regret, as such a sensible brute deserved to live." J. M. HAYWARD

Sidmouth, February 9

## Circular Rainbow seen from a Hill-top

CLIMBING, several summers ago, with three friends among the Coolin Hills in Skye, I was fortunate enough to witness phenomena similar to those described by Mr. Fleming in last week's NATURE (p. 310). Our shadows were apparently thrown against the precipitous side of a deep corry, distant 200 feet or perhaps more. They vanished and reappeared as thin mists passed through the corry, the sun shining continuously. We could not see each other's shadows unless close. The distance apart at which they became visible I do not clearly remember, but know it was approximately as one of my friends, Mr. W. A. Brown, writes:—"So long as we kept a few yards apart each could only see his own shadow, but when two were within arms' length a double shadow was visible to each, and on getting still nearer the shadows merged into each other." My estimate of the angle subtended by the diameter of the rainbow is 15°, that of my friend 10°. He adds, however, "I may be very far out in this." J. M. WHITE

Spring Grove, Dundee, February 5

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REFERRING to Mr. Fleming's letter in NATURE of January 31 (p. 310), I would state that many years ago, before Pontresina, in the Grisons, was so resorted to as it is now, I walked up the Piz Languard early one fine morning with an old smuggler and chamois hunter—the terms are synonymous on the frontier—named Colani. On the summit of the peak is a ledge of rock, on which I lay down for twenty minutes' sleep. I had been asleep but a few minutes when Colani woke me, and, with excuses and an expression of fright on his face, begged me to come with him to see something which he had never seen in his life before. We moved to the western edge of the peak. Below us were some thin clouds of mist curling about like vapour from a large cauldron. On these clouds appeared a circular rainbow and within it, as though in a gilded frame, were two figures—in fact, the shadows of ourselves.

"There are two of them now," cried Colani, and it was not until I told him to take off his hat and wave it, as I did mine, and he saw the action repeated by the figures, that he began to feel assured they were not "Geists." It was not the "Arch St. Martin," a Romansch name for a rainbow, which had frightened him, though it was the first time he had seen a circular one, but the appearance of the dark solitary figure had awakened his conscience, for some of his smuggling adventures had not been without bloodshed. The details of the phenomenon were the same as those described by Mr. Fleming, with the exception, perhaps, that the figures were more vivid and the whole spectacle of longer duration, owing to an unclouded sun.

A similar appearance has lately been seen on the Tonjale Range in Nevada, by Mr. R. A. Marr, of the Coast and Geodetic Survey. I subjoin his description of it, taken from a recent number of the *Mail*. T. R. MAYNARD

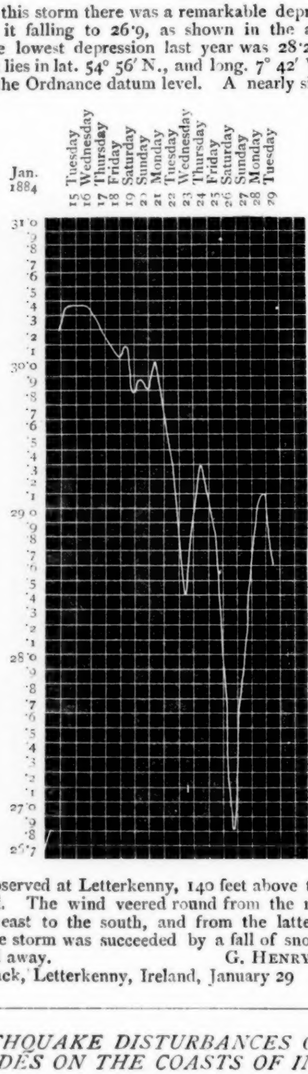
The Black Forest, February 7

"Suddenly, as I stood looking over the vast expanse beneath me, I saw myself confronted by a monster figure of a man standing in mid air before me, upon the top of a clearly-defined mountain peak, which had but the thin air of the valley below for a resting place. The figure was only a short distance from me. Around it were two circles of rainbow light and colour, the outer e faintly defined as compared with the inner one, which

bright and clear and distinctly iridescent. Around the head of the figure was a beautiful halo of light, and from the figure itself shot rays of colour normal to the body. The sight startled me more than I can now tell. I threw up my hands in astonishment, and perhaps some little fear, and at this moment the spectre seemed to move towards me. In a few moments I got over my fright, and then, after the figure had faded away, I recognised the fact that I had enjoyed one of the most wonderful phenomena of nature. Since then we have seen it once or twice from Jeff Davis Peak, but it never created such an impression upon me as it did that evening when I was doing service as a heliotroper all alone on the top of Arc Dome."

### The Storm of January 26

DURING this storm there was a remarkable depression of the barometer, it falling to 26·9, as shown in the accompanying chart. The lowest depression last year was 28·2 on Nov. 25. Lurgybrack lies in lat. 54° 56′ N., and long. 7° 42′ W. It is 225 feet above the Ordnance datum level. A nearly similar depression was observed at Letterkenny, 140 feet above the Ordnance datum level. The wind veered round from the north-west by north and east to the south, and from the latter by west to north. The storm was succeeded by a fall of snow, which has now melted away.




G. HENRY KINAHAN

Lurgybrack, Letterkenny, Ireland, January 29

## EARTHQUAKE DISTURBANCES OF THE TIDES ON THE COASTS OF INDIA

FOR some years past tidal stations have been established at various points on the coasts of India, from Kurrachee round *viâ* Cape Comorin and Adam's Straits to Calcutta, and on to Rangoon and Moulmein; also beyond these points, eastwards at Port Blair in the Andaman Islands, and westwards at Aden; but not anywhere in the Island of Ceylon, which happens—unfortunately for the interests of science—to be outside the administration of the Government of India. At each of the tidal stations an observatory has been established, containing a self-registering tide-gauge, and all requisite meteorological instruments, with a clerk in charge who tends the instruments, sets the driving clocks to true time—usually received telegraphically from Madras—and sends in daily reports to the supervising officer. That officer exercises a general superintendence over all the tidal stations, inspects them periodically, collates and analyses the observations, and deduces from them the values of the "tidal constants" for each port or point of observation; these constants enable future tides to be predicted, and tide tables to be prepared for the guidance of mariners; they are also otherwise valuable, in that they have thrown light on the question of the earth's rigidity, and on various other matters of scientific interest.

The operations have been carried on in connection with the Great Trigonometrical branch of the Survey of India. Major A. W. Baird, R.E., has been the supervising officer from their commencement in 1873 up to the present time, with the exception of an interval of a little more than a year, when he was on furlough in Europe, and Capt. J. Hill, R.E., first, and afterwards Major M. W. Rogers, R.E., officiated for him.

At certain of the Indian stations the registrations have twice indicated that the normal tides had been greatly disturbed by supertidal waves: first, on the occasion of the earthquake in the Bay of Bengal on December 31, 1881; and secondly, during the volcanic eruptions in the Island of Krakatoa, between Sumatra and Java, which occurred on August 27 and 28 last. The first disturbances do not appear as yet to have attracted much attention out of India; a full account of them is given in the General Report on the Operations of the Survey of India for 1881-82, and also in the *Proceedings of the Asiatic Society of Bengal* for March 1883. The second are now famous all the world over, not merely because of the havoc they are known to have produced on the spot and at the time, but also because of the effects they are believed to have produced on the condition of the atmosphere long afterwards and in far distant quarters of the globe. A report on the tidal disturbances at Indian stations which were caused by the eruptions at Krakatoa has been drawn up by Major Baird, and sent to me for communication to the Royal Society, and an abstract of it was read at the meeting of the Society on January 31.

I now propose to indicate certain points of similarity and others of dissimilarity between the recorded effects of the disturbing forces on the two occasions; for fuller details the reports themselves must be referred to.

The usual effect of an earthquake or volcanic eruption occurring at an island or under the bed of the sea is the transmission in all directions of an "earth-wave" and a "sea-wave"; the former travels with much greater rapidity than the latter, and may reach points which the latter does not reach; or it may die away and cease at points far short of those attained by the latter; which of the two will travel the greater distance depends generally on the structure and homogeneity of the strata through which the earth-wave is transmitted, and on the depth of water and configuration of the bottom over which the sea-wave passes.

On the occasion of the earthquake of December 31, 1881, the "centre of impulse" was situated under the bed of the ocean in the western portion of the Bay of Bengal; the shock of the earth-wave was very violent in the Andaman and Nicobar Islands, and along the entire length of the Madras coast up to Calcutta, and also far inland; it was followed by a succession of sea-waves which the tidal diagrams show to have arrived after the

earth-wave, at an interval ranging from half an hour at Port Blair (in the Andamans), the nearest station, to six hours at Dublat (in Sangor Island at the mouth of the River Hooghly), the furthest station at which such waves were certainly registered. At Rangoon, Moulmein, and various points in the Mergui Archipelago, the earth-wave was distinctly perceptible, though its shock was here much less violent; but no trace of a sea-wave has been met with at any of the tidal stations in this quarter; the belt of islands and shoals which extends from Cape Negrais to the Island of Sumatra, practically dividing the Bay of Bengal into two portions, must have formed a barrier to the sea-waves, for though great and numerous at Port Blair, they died away in the deep sea beyond, and in no case reached the eastern coast line.

The position of the earthquake in the Bay of Bengal was necessarily not a matter of observation as at Krakatoa; but it has been inferred by Major Rogers from the following facts. The moment at which the shock of the earth-wave was felt happens to have been recorded with considerable accuracy at three places, two on the west coast of the Bay, viz. the Madras Astronomical Observatory and the tidal station at False Point; the third on the east coast, Kisseraing, a principal station of the Great Trigonometrical Survey, where Major Rogers was actually at the moment observing a distant station in the field of the telescope of his theodolite. He reports that "he saw the earthquake before feeling it," for he first became sensible of its occurrence by noticing the object which he was observing appear to rise and fall in the telescope; he immediately examined the spirit-levels of his instrument, found they were violently agitated, and made a note of the time. Subsequently he ascertained that the shock he felt and those recorded at Madras and False Point must have occurred almost simultaneously, due allowance being made for the differences of longitude. Therefore, assuming the earth-wave to have travelled from the centre of impulse with the same velocity in all directions, the centre would be near that of the triangle joining the three points of observation, but probably a little to the south, towards the line joining Port Blair and Negapatam, the stations at which the tidal disturbances were the greatest.

Having thus ascertained the probable position of the centre of impulse, Major Rogers proceeded to ascertain the probable time of the earthquake. Here again he was favoured by his facts. It so happened that his assistant, Mr. Rendell, had just completed an inspection of the tidal station at False Point, and was at work on a line of levels a few miles away, when he felt a violent shock of earthquake; he noted the time; the clerk at the station also felt the earthquake, and noted that the observatory was much shaken; afterwards it was found that at the time recorded by Mr. Rendell the pencil of the tide-gauge had been vibrating very sensibly on the diagram; the vibration must have been caused either by the shaking of the observatory, or by a forced sea-wave such as is sometimes produced momentarily in shallow waters by a passing earth-wave. The great sea-wave which was transmitted from the centre of impulse arrived 3 hours 18 minutes afterwards. Now there can be no question that the vibration mark on the diagram correctly registers the moment at which the earth-wave reached False Point; Major Rogers therefore conjectures, with much probability, that a similar very prominent vibration mark on the Port Blair diagram registers the moment of the arrival of the earth-wave at Port Blair; thirteen minutes after the time thus registered Major Rogers felt the earthquake at Kisseraing, and as the distance between the two points is 400 miles it may be inferred that the earth-wave travelled with a velocity of about 1800 miles an hour. With this velocity, the distance of the assumed centre of impulse from either of the three surrounding stations, and the time of the occurrence of the earth-wave at either station, Major Rogers calculates the time of the original disturbance when both the earth-wave and the sea-wave were initiated. Comparing this time with that of the arrival of the sea-wave at his stations, he obtains the following velocities for the sea-wave: to Port Blair 360 miles an hour, to Madras and Negapatam 240, to False Point 180, and to Dublat 120. The average depth of the sea is known to diminish in every instance of diminished velocity.

The sea-wave here specifically referred to was the first and generally the greatest of the supertidal waves; its amplitude from trough to crest was a maximum, 36 inches, at Negapatam, and 30 inches at Port Blair; it was always positive, the crest preceding the trough and raising the sea-level. The latter point is to be specially noticed because the first result of the great eruption at Krakatoa was the reverse of this, namely, a negative wave or general lowering of the sea-level at the stations of observation, as will be shown more fully further on. Secondary sea-waves followed the first, disturbing the normal tides for some hours: their greatest duration was twenty-five hours at Port Blair, the nearest tidal station to the centre of impulse. A single earth-wave of a few seconds' duration is all that appears to have been perceived at the tidal stations; possibly, therefore, the whole of the tidal disturbances were due to a single earthquake.

Proceeding now to the eruptions at Krakatoa, we find that while there is no uncertainty as regards their locality, and there is evidence of one great eruption far exceeding all the others in violence, there is as yet no certain information of their number nor of the times at which any of them, even the greatest, occurred. No earth-waves appear to have reached India; but sea-waves of more or less magnitude were transmitted to all the tidal stations on both coasts of the peninsula, and not alone to those on the east coast, as on the former occasion; they were also transmitted far beyond, to Aden, the Mauritius, and the south-east coast of Africa, as shown in Major Baird's report. Lately it has been announced that traces of the sea-waves have been discovered at French tidal stations on both coasts of the Atlantic.

The principal facts set forth by Major Baird are the following:—

1. Distinct evidence of tidal disturbance was met with at twelve of the seventeen Indian tidal stations, including all which were fairly placed to receive the force of the impulse from Krakatoa; but, as in the previous instance, no disturbance was perceived at the stations on the east coast of the Bay of Bengal.

2. The first result of the great eruption at Krakatoa was a negative supertidal wave, or general fall of the sea-level, at Major Baird's stations and also at the Mauritius.

3. This negative wave was succeeded by a great positive wave, at an interval ranging from seventy-five minutes at Negapatam, the station nearest Krakatoa, to twenty-four minutes at Aden, the most distant station.

4. Supertidal waves of greater or less magnitude were registered at the Indian stations some hours before the negative wave of the great eruption, showing that there must have been antecedent minor eruptions. They appear at Aden about three hours before the negative wave, and eighteen hours before at Negapatam, showing that the explosions were at first comparatively feeble, affecting only the nearer stations; but afterwards they increased in intensity and became sensible even at Aden, a distance of over 4000 miles.

5. Waves of amplitudes ranging from a maximum of 22 inches at Negapatam to a maximum of 9 inches at Aden were registered at all the more favourably situated stations. The first was the positive wave immediately succeeding the primary negative wave, and it was generally of a greater amplitude than any other wave, but in a

few instances it was succeeded by greater waves. The succeeding waves maintained considerable amplitudes—not less than half the maxima values—for about twelve hours, appearing at intervals of one or two hours apart at all the more prominent stations. They were succeeded by wavelets gradually diminishing in size, but continuing for some time, being traceable on the diagrams for August 29 and 30, the second and third days after the great eruption. It is noticeable that they ceased first at Port Blair and Negapatam, the two nearest stations, and last at Aden, the farthest station.

6. Loud reports, resembling the firing of distant guns, were heard at Port Blair on August 26 and 27, and being supposed to be signals from a vessel in distress a steamer was sent out in search of the vessel; similar reports were heard on the 26th in Ceylon.

These facts show that the great eruption at Krakatoa was preceded by minor eruptions sufficiently powerful to produce effects which were sensible at a distance of upwards of 4000 miles; also that it was probably followed by minor eruptions, to the influence of which the long-protracted continuance of tidal disturbance is due.

The time at which the great eruption occurred is still not known with any precision. Major Baird has endeavoured to calculate it from the following data: he was informed by Her Majesty's Consul in Java that the first great (positive) wave reached Batavia at 12h. 10m. local time on the afternoon of August 27; as the distance from Krakatoa by sea is 105 miles, and the average depth of the sea about 186 feet, he infers from the table of the velocity of free tide waves passing over seas of different depths, in Sir George Airy's article on "Tides and Waves" in the *Encyclopedia Metropolitana*, that the wave must have taken about two hours to reach Batavia, and therefore that it must have started at 10.5 a.m. Krakatoa time, allowing five minutes for the difference of longitude. Another estimate has been recently furnished by General Strachey in a paper—read before the Royal Society—on the "Barometrical Disturbances which passed over Europe between August 27 and 31"; General Strachey connects these disturbances with the great eruption at Krakatoa, and infers, from the recorded evidence of the times of transit of the barometric waves over the European observatories, that the initial barometric rise occurred at 9h. 24m. Krakatoa time on the morning of August 27. Now we have seen that the first effect of the great eruption on the ocean was the production of a negative wave which preceded the great positive wave by an interval of seventy-five minutes at Negapatam, and twenty-four minutes at Aden; if then we assume that the interval was somewhat more than seventy-five minutes at Krakatoa itself—as is to be inferred from the fact that wherever registered it increases as the distance from the centre of impulse diminishes—General Strachey's and Major Baird's determinations will be seen to corroborate each other very closely; indeed, considering the absolute independence of the two methods of deduction, the facts of observation being in one instance derived from the atmosphere, in the other from the ocean, the coincidence between the results is very striking.

Major Baird has calculated the velocities with which the great positive wave travelled from Krakatoa to the more important of his own stations, and also to Port Louis in the Mauritius, and Port Elizabeth in South Africa.[1] Starting with the assumption that the wave left Krakatoa at 10·5 a.m., August 27, local time, he finds that it attained its maximum value, 467 statute miles per hour, in transit to both Port Louis and Port Elizabeth. Considerable interest attaches to this determination, in that it is identical with Sir George Airy's tabulated value of the velocity of a free tide-wave passing over an ocean 15,000 feet deep, which is supposed to be the average depth of the ocean in this direction; moreover, the fact that the same velocity is obtained for both the ports, and that the nearer of the two is only 3400 miles from Krakatoa, while the other port is 5450 miles distant, indicates that there is probably no material error in Major Baird's adopted time of starting. The velocity of the wave in all other directions is less, viz. to Galle 397 miles, to Negapatam 355 miles, and to Aden 371 miles. The velocities are necessarily computed on the assumption of a uniform rate of progress from the origin to the point reached; but each of the slower waves must have coincided with the wave which impinged on Ports Louis and Elizabeth for a considerable distance in the early portion of its course, and it must then have travelled with the same high velocity; afterwards, on passing over shallower seas, the velocity must have much diminished, and very possibly it may have fallen to the smaller velocity values which Major Rogers has calculated for the sea-waves in the Bay of Bengal, on the occurrence of the earthquake of December 31, 1881.

The Admiralty chart of the Eastern Archipelago shows that Krakatoa is situated at the focus of what may be regarded as a parabolic figure, formed by the contiguous portions of the coasts of Java and Sumatra; the axis of the figure is directed towards the Indian Ocean. Thus the waves generated by an eruption at Krakatoa would be mostly propelled towards that ocean, both directly and by reflection from the coasts; but near the apex of the parabola there is an opening, the Straits of Sunda, through which a great wave passed, carrying widespread destruction for some distance beyond along the contiguous coasts. This wave may have impinged with great force on the south-west corner of the Island of Borneo, which is on the prolongation of a straight line drawn from Krakatoa through the Straits. But it did not reach Singapore, where a tide-gauge is established, and which is within a third of the distance of the nearest Indian station from Krakatoa; the Master-Attendant at Singapore reports that the gauge shows "no difference whatever in the tide." This is obviously due to the fact that the wave which passed through the Straits of Sunda had but a shallow sea to advance over towards Singapore, and its course must have been greatly impeded by numerous islands and shoals and the narrow straits and passages between them. For similar reasons, and because the axis of the parabola in which Krakatoa is situated is pointed towards the Indian Ocean, it is probable that the effects of the eruptions were not conveyed to anything like so great a distance along the numerous groups of islands to the east and into the Pacific Ocean.

J. T. WALKER

[1] For these ports he employs the data published in NATURE, vol. xxviii. p. 626.

## *THE INDIAN SURVEY*[1]

THIS is the fifth report of the amalgamated Department of Surveys under the Government of India. It is divided into two parts with an appendix. Part I. gives a summary of the operations of the great trigonometrical, the topographical, and revenue survey parties; also of the geographical, geodetic, and tidal, and levelling operations. Part II. describes the operations at the Head-Quarters Offices, viz. the Surveyor-General's Office, the Revenue Survey Office, the Lithographic Office, the Photographic Office, and the Mathematical Instrument Department, all in Calcutta; and of the Trigonometrical Survey Office in Dehra Dun. Index charts, coloured maps, and sketches showing the present state of this very important department accompany this report; to which is prefixed, as frontispiece, a "Specimen of *Heliogravure* by Major Waterhouse's Process," which invites the

[1] "General Report on the Operations of the Survey of India during the year 1881-82." Prepared under the superintendence of Lieut.-General J. T. Walker, C.B., R.E., F.R.S., &c., Surveyor-General of India. (Calcutta, 1883.)

special attention of photographers and engravers. An appendix, separately paged, of 120 pages, completes the volume, and consists of extracts from the narrative report of the executive officers in charge of the survey parties and operations.

This report is distinguished from previous ones by announcing the completion of the great triangulation on the lines originally marked out in 1830 by Col. Everest, which affords the Surveyor-General, in his introductory statement, an opportunity of giving a brief but interesting history of this great undertaking from its commencement in the year 1800, in Southern India, by Major Lambton, on the recommendation of the Hon. Col. Wellesley, afterwards the Duke of Wellington. The object of this so-called "Mathematical and Geographical Survey" was then stated to be "to determine the exact positions of all the great objects best calculated to become permanent geographical marks to be hereafter guides for facilitating a general survey of the peninsula," and further, that in the interests of general science it would have to be executed with the utmost possible precision, and be supplemented by astronomical determinations of position, with a view to the requirements of geodesy.

The operations between the years 1800 and 1825 may be briefly described as consisting of a network of triangulation over Southern India, and through the middle of which a principal chain of triangles was carried in a meridional direction from Cape Comorin up to Sironj in Central India. This chain forms that which is now known as Lambton and Everest's Great Arc. Col. Lambton died in 1823, and was succeeded by Col. Everest, who, two years afterwards, proceeded to Europe, spending four years in supervising the construction of new instruments—great theodolites, astronomical circles, standards of length, and compensation bars for base-line measurements, for employment in extending and revising the Great Arc, the importance of which was recognised by all men of science in Europe.

Returning to India in 1830, Col. Everest recommended the abandonment of the network system of triangulation and the substitution instead of what he called the "gridiron" system, consisting of meridional chains of triangles tied together at their upper and lower extremities by longitudinal chains. The meridional chains were to be constructed at intervals of about one degree apart, while longitudinal chains would follow the parallels of Calcutta, Bombay, and Madras, and thus run at intervals of from five to six degrees apart. The external chains of the gridiron would of course follow the British frontier lines and the coast lines, and all grounded on ten base lines measured with the Colby apparatus of compensation bars and microscopes. This programme of operations was approved by the Government of India and Court of Directors, and has furnished the guiding lines on which the principal triangulation has been executed during the period of almost exactly fifty years which has since elapsed. For geodetic purposes, the amount of principal triangulation is now ample. Outside the limits of India proper, the recently completed chain of principal triangles, called the eastern frontier series, is a valuable contribution to geodesy and geography.

Thus the great work of the principal triangulation of India is now an accomplished fact. Commenced in 1800, under the auspices of the Madras Government, it was carried on, almost alone, by Major Lambton, until 1818, when the Marquis of Hastings, then Governor-General, placed it under the control of the supreme Government, and Capt. Everest was appointed assistant to Major Lambton. In 1832 additional officers were appointed, and by the year 1840, when the northern section of the Great Arc was completed, the *personnel* sufficed for the equipment of six triangulation survey parties, which number has been uniformly maintained from that time onwards until gradually diminished on the completion of the successive chains of triangles. The operations have been uniformly and consistently supported by successive Governments of India with equal liberality and constancy, and to whom it must be a source of much satisfaction to know that this great work of permanent peaceful usefulness will assuredly take the highest rank as a work of scientific labour and skill.

It is stated that there are 3472 principal stations. On the plains they are constructed in the form of towers, rising from 20 to 40 and even 60 feet above the ground, and usually about 16 feet square at base, with an isolated central pillar for the instruments to rest on. On hills and mounds and other eminences the central pillar, always of masonry, is raised 2 to 4 feet above the ground level, and is surrounded with a platform of earth and stones. Mark-stones, engraved with circle and central dot to define precisely the station point of observation, are inserted at the surface and at the base of the pillar. The stations, scattered over 338 British districts and native states, are placed under the protection of local officials, each of whom is required to send annual reports of the condition of the stations within his district. Repairs are effected when necessary, and if so maintained by future generations of officials, the duration of the stations should be coeval with the hills and plains on which they stand, and be of lasting utility.

The field operations of measurements of base-lines and angles of the principal triangulation being completed, the simultaneous reduction of the vast number of such facts, acquired over all India, by many individuals and during a period of many years, to a harmonious whole, was obviously impossible. Thus it became necessary to divide the triangulation of India proper into five sections; and even then the simultaneous reduction of the numerous facts of observation collected together in each group was a work of enormous labour, necessitating, as stated by the most eminent living authority (Col. Clarke, C.B., *Geodesy*, p. 257), "the most elaborate calculations that have ever been undertaken for the reduction of triangulation by the method of least squares." The final results of the first section are given in vols. ii., iii., and iv. of the "Account of the Operations of the Great Trigonometrical Survey of India," published in 1879 (vol. i. is devoted to base-lines, and vol. v. to pendulum operations); those of the second section in vol. vi., published in 1880, and those of the third in vols. vii. and viii. will be shortly published. The simultaneous reduction of the fourth section is now completed. The final reduction of the last section has not yet commenced, nor has the recently completed eastern frontier series.

The requirements of geodesy necessitate astronomical observations for the determination of latitude and azimuth and electro-telegraphic observations for the determination of differences of longitude at several stations of the principal triangulation. These have already been completed to a considerable extent; and further operations of this nature are in progress by two small astronomical parties attached to the geodetic branch of the department, and by whom all the operations subservient to geodetic science should be completed in the course of a few years. An extensive series of pendulum observations for investigations of gravity and the figure of the earth, taken chiefly at stations of the principal triangulation, has been completed and connected with the groups of corresponding observations in other parts of the globe. Long lines of spirit levels have been, and are still being carried on in connection with the principal triangulation, from the sea to the base-lines in the interior, and from sea to sea across the peninsula; they rest on determinations of the mean sea level, which have been and are being made at tidal stations on the coasts, and which promise to furnish most important data by means of which our knowledge of the constitution of the earth's mass may be extended.

Reference can only be here made to the report for most

interesting information as to the progress of the thirteen topographical parties, the two *Mouzawar*, or village survey parties, and the six cadastral or field survey parties, whose duties now include, as an experiment, the reecording of particulars about each field; thus reducing the cost of preparing the "Record of Rights" for the Board of Revenue. The geographical reconnaissance and trans-Himalayan explorations are replete with curious information to every student of nature, and of the habits and customs of the frontier hill tribes and peoples. The perusal of this report increases, if possible, our good opinion of the skill and devotion to duty of the several officers, and of the marked ability of the administration of this department by General Walker, and which it is most pleasing to find so handsomely acknowledged by the Government of India.

## *ZOOLOGY AND BOTANY OF ALASKA*[1]

THE United States Revenue cutter *Corwin* went on a cruise in 1881 to Alaska and the Arctic Ocean. The main object of the voyage was to search the various accessible portions of the Arctic coasts for traces of the *Jeannette* and two missing whaling vessels which were lost the same season that the *Jeannette* entered the ice. Leaving St. Michael's on June 21, Behring's Sea was crossed to St. Lawrence Island and Plover Bay on the Siberian coast; then the *Corwin* went along this coast through the Straits and north-west to the vicinity of Nordenskjöld's winter quarters, where a sledge party, which had been left there earlier in the season to search the coast in that district, was taken on board; it then returned to St. Lawrence Island and St. Michael's. After a short delay it again proceeded to the Arctic, touching at all the islands in Behring's Straits, visiting in succession the entire Alaskan coast line from Behring's Straits to Point Barrow, including Kotzebue Sound, and on the Siberian shore from the Straits to North Cape. It also cruised along the edge of the ice pack, visiting Herald and Wrangel Islands—almost unknown masses of land—and, returning homewards, some time was spent at Ounalaska in the Aleutian Islands fitting for the voyage to San Francisco, which was reached in October.

As one of the results of this cruise, we have a series of notes and memoranda, medical and anthropological, botanical and ornithological, published by order of the House of Representatives at Washington.

The medical and anthropological notes of Alaska are by Dr. Irving C. Rosse. The health of the ship's crew was fairly good throughout the voyage, very careful precautionary measures being observed: for the usual habit of deluging the decks above and below every morning with water, a system of scraping and dry scrubbing was substituted with excellent results, and the decks were only wetted once or twice a month on fine days. Good water was procured nearly everywhere in the Arctic, and it is noted as of unusual excellence at Cape Thompson and at Herald and Wrangel Islands. The weather was mostly wild, with snow and hail; in the latter part of June at St. Michael's the sun was found almost overpowering, although the thermometer registered but 60°. Dr. Rosse gives a sketch of the diseases peculiar to the aboriginal population, especially of an epidemic of pneumonia which prevailed at Ounalaska. He declares "that there is an absolute consensus of opinion both among the executive and medical officers of late Arctic expeditions in regard to the judicious use of alcoholic beverages," and that though himself of abstemious habits, yet the facts observed "warrant him in testifying to the undeniable good effects of whisky when served out to the crew after unusual fatigue and exposure." On reaching St. Lawrence Bay, Siberia, a native speaking a little English was at his own request taken on board; the bustle and stir brought on a state of sleeplessness, and his state of mind was not improved on seeing the collection of skulls on board, nor by the chaff of the forecastle men, who tried to persuade him he was to be brought to San Francisco as an anatomical curiosity. As a result he stabbed himself dangerously in the left chest, and then leaped overboard; a boat being alongside, he was promptly rescued. The knife was found to have entered several inches, and blood and air were escaping from the wound. The symptoms were such that, writes Dr. Rosse, "the patient ought to have promptly perished, notwithstanding the treatment," but in a few days the patient was landed at Plover Bay, where he recovered sufficiently to start on foot for his home over a rugged mountain way 150 miles distant. "Wounds seem to heal uncommonly well in the Arctic, a fact doubtless owing to the highly ozonised condition of the atmosphere, and the absence of disease germs and organic dust."

Dr. Rosse's anthropological notes on the natives met with are of some importance, though his conclusions based on these may not always be acceptable. Referring to the prevalence of tattooing among the Esquimaux women, he gives a figure of strange design seen on the cheeks of a woman of St. Lawrence Island. Some drawings of crania are given, but we have failed to find any detailed account of them.

The botanical notes on Alaska are by John Muir. There is no line of perpetual snow on any portion of the Arctic regions known to explorers. Every summer the snow disappears not only from the low sandy shores and boggy tundras, but also from the mountain tops; for nearly three-fourths of the year the plants lie buried under it, but they awake up in June and July to a vigorous growth, and on the drier banks and hills about Kotzebue Sound, Cape Lisbourne, and elsewhere, many species show but little climatic repression, growing during the long summer's day tall enough to wave in the wind, and to unfold a rich profusion of flowers. A list of the species found at the following localities is given—St. Michael's, Golovin Bay, Kotzebue Sound, and Cape Thompson, where a new species of Erigeron was found (*E. muirii*, Gray). On Herald Island sixteen species of flowering plants were gathered. At Wrangel Island, from an area of about half a square mile, twenty-seven species of flowering plants were collected; they all occurred in separate tufts, leaving the ground between them bare and raw as that of a newly ploughed field. Some portions of the coast, however, farther south, presented a greenish hue, as seen from the ship, at a distance of eight or ten miles, owing no doubt to vegetation growing under less unfavourable conditions than at the point the *Corwin* touched at.

The birds of Behring's Sea and the Arctic Ocean are described by Mr. E. W. Nelson; many of the breeding quarters of North American birds are given, and details are also added of some of the rarer forms met with. A fine adult male Siberian Wagtail (*Motacilla ocularis*, Swinhoe) was taken at Plover Bay the last day of June; it was in perfect breeding plumage. A specimen of *Lanius cristatus* was picked up dead on Wrangel Island. Strictly an Asiatic bird, it must have reached this distant spot during some storm, and died of starvation or exposure. A fine adult female, in breeding plumage, of *Eurinorhynchus pygmæus*, was taken at Plover Bay, and several others were seen. A specimen of *Rhodostethia rosea* in immature plumage was obtained at St. Michael's, and reference is made to three fine specimens secured by Mr. Newcomb during the drift of the *Jeannette*, which are now in the Smithsonian collection, one of which still retains its extremely rich peach-blossom pink so characteristic of this the most beautiful of the gulls.

[1] "Cruise of the Revenue Steamer *Corwin* in Alaska and the North-West Arctic Ocean in 1881. Notes and Memoranda, Medical and Anthropological, Botanical and Ornithological." (Washington: Government Printing Office, 1883.)

A list of the fishes known to occur in the Arctic Ocean, north of Behring's Straits, by Tarleton H. Bean, is appended. The list is based exclusively upon specimens in the United States National Museum, and is acknowledged to be incomplete; it only contains twenty-one species, eight others being added as "properly belonging to the fauna." No details beyond the localities where found are given.

## *SOUND-MILLS*

AFTER the notable researches of Crookes on radiation, which culminated in the discovery of the radiometer, or light-mill, it was a natural transition of thought which suggested to several minds almost simultaneously the possibility of devising an apparatus which should rotate under the influence of sound-waves as does the radiometer under the influence of the rays of light and heat. Such instruments were indeed devised independently about six years ago by Lord Rayleigh, by Prof. Alfred M. Mayer of Hoboken, by Mr. Edison, the well-known inventor, by Prof. Mach of Prague, by Dr. A. Haberditzel of Vienna, and by Prof. V. Dvorák of the University of Agram (in Croatia). These researches, though of great scientific interest, have been somewhat overlooked in the rush of scientific inventions during the intervening years. During the course of the past year,

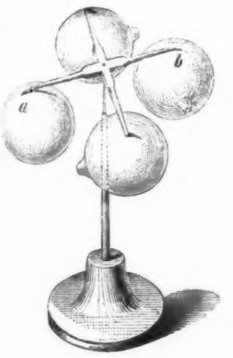



FIG. 1.


FIG. 2.

however, Dr. Dvorák has given to the world, in the pages of the *Zeitschrift der Instrumentenkunde* (vol. iii. Heft 4), a detailed account of his experiments, together with figures of various pieces of apparatus hitherto undescribed. We propose to give a *résumé* of the principal points of Dvorák's researches.

Four kinds of sound-mills are described by Dvorák, two of them depending on the repulsion of resonant-boxes or cases, and two others on different principles.

The first of these instruments is depicted in Fig. 1, and consists of a light wooden cross, balanced on a needle point, carrying four light resonators made of glass. These resonators are hollow balls of 4·4 cms. diameter, with an opening of 0·4 cm. at one side. They respond to the note $g'$ (=392 vibrations). When the note $g'$ is forcibly sounded by an appropriate tuning-fork, the air in each of the resonators vibrates in response, and the apparatus begins to rotate. As a resonator will respond when placed in any position with respect to the source of sound, it is clear that one single resonator properly balanced should rotate; and this is found to be the case, though, naturally, the action is more certain with four resonators than with one.

Before proceeding to the other forms of sound-mill devised by Dvorák, it may be well to explain briefly the cause of the phenomenon, and to describe Dvorák's particular method of exciting the appropriate sound Dvorák has pointed out, as indeed has been done elsewhere both by Lord Rayleigh and by Prof. A. M. Mayer, that, when sounds of great intensity are produced, the calculations which are usually only carried to the first order of approximation cease to be adequate, because now the amplitude of motion of the particles in the sound-wave is not infinitely small as compared with the lengths of the sound-waves themselves. Mathematical analysis shows that under these circumstances the mean of the pressures in the condensed part and in the rarefied part of the sound-wave is no longer equal to the undisturbed atmospheric pressure, but is always greater. Consequently at all nodal points in the vibrations of the air in tubes or resonant-boxes the pressure of the air is greater than elsewhere; and therefore any resonator closed at one side and open at the other is urged along bodily by the slight internal excess of pressure on the closed end. The apparatus, Fig. 1, therefore rotates by reaction, in the same way as Hero's primitive steam-engine rotated, though the reaction is due to a different cause.

To produce vibrations of sufficient intensity Dr. Dvorák employs heavy tuning-forks mounted on resonant-cases, and excited electrically. For this purpose he places between the prongs of the fork an electromagnet constructed on the following plan. Two plates of iron separated by a sheet of paper are used as a core. They

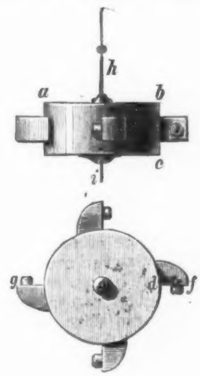



FIG. 3.

are cut of such a breadth as to lie between the prongs without touching them. This core is overwound with insulated copper wire, as shown at E, Fig. 2, and the electromagnet is then mounted by a bent piece of wood, *abc*, upon the sounding-box, K, of the fork. The wires are connected in a circuit with a battery, and with the electromagnet of a self-exciting tuning-fork of the same note. Dr. Dvorák is extremely particular about the arrangements of the resonant-boxes of his tuning-forks. They must not touch the table, the arm *a b c* being clipped at about the point *b* in a firm support. Moreover the resonant-boxes themselves require to be specially tuned, for all are not equally good. Dr. Dvorák points out that, beside the tone of the fork, and the tone of the air column in the cavity of the box, there is also a tone proper to the wood of the box itself, which in most of the forks used in acoustic researches is too base, the wooden walls being too thin. To hear this tone the prongs of the fork should be damped by sticking a cork between them, and the cavity should be filled with cotton-wool, while the wooden box is gently struck with the knuckle or with a cork hammer. It is important that the wood-tone should be tuned up to coincidence with the tone of the fork and with that of the air in the cavity. Dr. Dvorák himself used the box depicted further on in Fig. 6, in which drawing F is the socket into which the stem of the fork

was screwed. The wood was tuned by planing it away at the top and bottom, while the air cavity was tuned by enlarging the circular opening in front. In the later researches the box stood on four feet made of india-rubber tubing. The note of the fork so mounted was very strong. At 40 cms. distance it would set the sound-mill in motion.

Dvorák's second apparatus, a "rotating resonator," consists of a short cylindrical box, constructed of stiff glazed paper, having four projections, shown in plan and elevation in Fig. 3, each of which bears at its side a short open tube of paper. It is, in fact, a resonator with four openings, arranged so that it can be hung upon a silk fibre. A fine needle projects also below to steady the

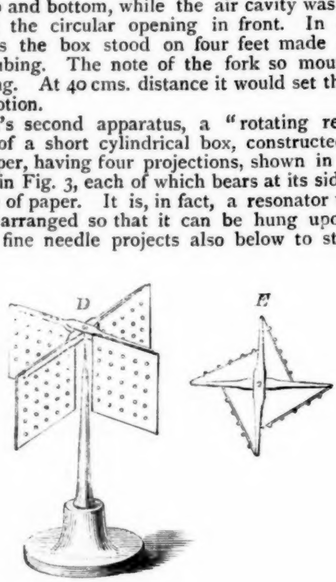



FIG. 4

motion during its rotation, which occurs whenever the apparatus is brought near to the sounding-fork. For the note $g'$ the dimensions were: diameter, 7 cms.; height, 3·6 cms.; diameter of openings, 0·6 cm.

The third apparatus is the "sound-radiometer" described by Dvorák before the Imperial Viennese Academy in 1881. Its cause of action is less readily explained, though its construction is even more simple. Its form is shown in Fig. 4, D; there being, as before, a light cross of wood, pivoted by a glass cap upon a vertical needle. To the four arms of the cross are cemented four pieces of fine white card, about 0·08 cm. thick, perforated with holes which are depressed conically at one side, and raised at the other. These holes may be made by punch-

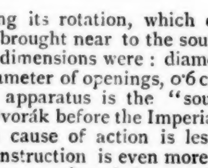



FIG. 5.

ing the card upon a lead block with a steel perforating-punch of the form shown in Fig. 5, A, the dimensions of which are: $ab = 0·38$ cm.; $cd = 0·2$ cm. The holes should be from 0·6 to 0·65 cm. apart from one another. When a card so perforated is held in front of the opening of the resonant-box of the tuning-fork it is repelled if the smaller ends of the conical holes are toward the box; or is attracted if the wider openings are toward the box. A better, but less simple, way of perforating the cards is by the use of the conical steel punch shown in Fig. 5, B, and the matrix, Fig. 5, C. The angle of the cone is 55°, and the narrow projecting nose of steel is 0·2 cm. The card should be damped, laid on the matrix C, and the hole pierced by two or three blows upon the die. Dr. Dvorák prefers this plan: it throws up a high burr or edge behind the conical hole, and such perforations are more effective. The cards may be varnished, and are then mounted upon the cross. The rotations are more rapid if the cards are set on obliquely in the fashion shown in Fig. 4, E, the burred sides being outwards. Cards with twenty-five perforations so mounted rotate briskly when the "mill" is set in front of the resonant-box.

The fourth apparatus of Dvorák is called by him an "acoustic anemometer." It is shown in Fig. 6. This is merely a little "mill" of simple construction, the vanes being small pieces of stiff paper or card slightly curved. The sounding-box previously described is placed a little way from it, and between them is held an ordinary Helmholtz's resonator, with its wide mouth, $b$, turned toward the box, and its narrow opening, $a$, toward the mill. From what has been previously said it will be understood that the internal increase of pressure in the resonator at $a$ has the effect of driving a jet of air gently against the sails of the mill, which consequently rotates. Dr. Dvorák also suggests that this two-aperture resonator may be replaced by one having but one aperture, as shown at R, with its

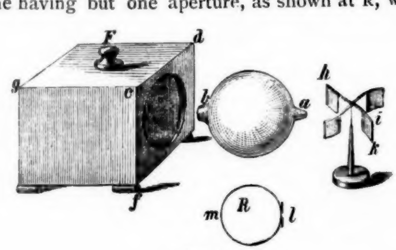



FIG. 6.

open side, $l$, turned towards the mill. This resonator is formed of a glass ball cut away at one side and cemented to a glass plate having a small hole at the centre. It may be remarked that when the air ejected from the mouth of this resonator is examined by the method of mixing smoke with it, and then viewing it through slits cut in a rotating disk, the currents are seen to consist of a series of vortex-rings.

A second kind of "acoustic anemometer" may be made by taking a card pierced with 100 conical holes, as previously described, and placing this between the resonant-box and the "mill." The latter rotates in the wind which passes through the conical holes.

Space does not admit of a comparison being drawn between these instruments and those of Mayer, Mach, and others, which are very closely akin in their design and mode of action, interesting though such a comparison might be. Nor can we here compare the action of these instruments with the "phonomotor" with which Mr. Edison literally accomplished the feat of talking a hole through a deal board. But this remarkable machine was a purely mechanical toy, which converted the vibrations of the voice, by means of a very finely-cut ratchet-wheel, into a motion of rotation round an axis.

SILVANUS P. THOMPSON

## NOTES

IN the last week British science has sustained a great loss in the death of Mr. Thomas Chenery, the editor of the *Times*. During his all too short reign the leading journal of Europe has been in strict harmony with the real progress of humanity, instead of being merely a chronicle of "politics" and "society," and day by day it has been wonderful to watch with what continuous well-balanced vigour and skill the general public has been made interested in the victories achieved in the domains of science, literature, and art, as only a daily journal can interest it.

Never before in the history of daily journalism in any country did science receive the recognition which Mr. Chenery accorded to it. Mr. Chenery was not only a great scholar, but the nearest approximation to an admirable Crichton that we have known, and in this we find the secret of his skill as an editor. So many-sided was he that whether teaching Arabic at Oxford as Lord Almoner's Professor; taking his part in the revision of the Old Testament; acting as Special Correspondent in the trenches in the Crimea; at his post as Editor of the *Times* or in private life, he won the admiration of all who knew him by his deep knowledge and splendid modesty. He was a perfect friend, and gained the respect and love of all who came into contact with him.

DR. JOHN HUTTON BALFOUR, Emeritus Professor of Medicine and Botany in the University of Edinburgh, Regius Keeper of the Royal Botanic Garden, and Queen's Botanist for Scotland, died on Monday at Inverleith House, Edinburgh. He was born in 1808. Dr. Balfour was the father of Prof. Bayley Balfour, whose appointment to the vacant Chair of Botany at Oxford we announce to-day. We hope to say more about the late Prof. Balfour next week.

THE death is announced of the distinguished American geographer, Prof. Arnold Henry Guyot. He was born at Neuchâtel, Switzerland, on September 28, 1807. He studied at Neuchâtel, Stuttgart, and Carlsruhe, and at the last-named place formed a close friendship with Agassiz, with whom he studied natural science. In a tour through Switzerland in 1838 he first discovered the laminated structure of the ice in glaciers, and showed that the motion of the glacier is due to the displacement of its molecules. Agassiz, Forbes, and others afterwards confirmed these discoveries. For seven successive summers Guyot now investigated the distribution of erratic boulders, tracing them on both sides of the Central Alps, in Switzerland and Italy, over a surface 300 miles long and 200 miles wide, and delineating eleven different regions of rocks. Their vertical limits and the laws of their descent were determined by means of more than 3000 barometrical observations; and the characteristic species of rock of each basin were tracked step by step to their source. In the United States he was employed by the Massachusetts Board of Education to deliver lectures in the normal schools of the State, and before the teachers' institutes, and by the Smithsonian Institution to organise a system of meteorological observations. In 1855 Guyot was appointed Professor of Physical Geography in the College of New Jersey at Princeton, which post he retained till his death. He was awarded a medal for his researches at the Vienna International Exhibition of 1873.

THE Royal Society has appointed a committee, consisting of Sir F. Evans, Prof. Judd, Mr. Lockyer, Mr. R. H. Scott, General Strachey, and Mr. G. J. Symons, with power to add to their number, to collect the various accounts of the volcanic eruption at Krakatoa, and attendant phenomena, in such form as shall best provide for their preservation and promote their usefulness; and a sum of 25*l.* has been placed at their disposal for this purpose. In connection with this we direct attention to the letter of Mr. Symons in our Correspondence Columns.

THE following note has been sent us from the Meteorological Office:—"We have received notice of the establishment of a system of storm and weather warnings on the Spanish coast. The warnings are based upon observations received from stations reporting daily by telegraph to the Marine Observatory at San Fernando, which is superintended by Capt. C. Pujazon of the Spanish Navy. This institution also issues a daily weather report and chart."

THE "Johns Hopkins University Circulars" have become an important medium for communicating briefly the results of research in all departments in connection with the many-sided institution which issues them. Doubtless they are to be found at the leading scientific centres in this country, and are always well worth looking into. The number for January contains a brief report of the meetings in connection with the departure of Prof. Sylvester from America; how highly he was appreciated there is evident from the following:—On the afternoon of December 20 the academic staff of the University met in Hopkins Hall, by invitation of the President, and after a brief review by Dr. Story of the mathematical lectures here given from 1876 to 1883, and a like review by Dr. Craig of the contributions printed in the *American Journal of Mathematics*, Prof. Gildersleeve read the following paper, which, on motion of Prof. Rowland, was adopted by the meeting as an expression of their respect and good will. "The teachers of the Johns Hopkins University, in bidding farewell to their illustrious colleague, Prof. Sylvester, desire to give united expression to their appreciation of the eminent services he has rendered the University from the beginning of its actual work. To the new foundation he brought the assured renown of one of the great mathematical names of our day, and by his presence alone made Baltimore a great centre of mathematical research. To the work of his own department he brought an energy and a devotion that have quickened and informed mathematical study not only in America, but all over the world; to the workers of the University, whether within his own field or without, the example of reverent love of truth and of knowledge for its own sake, the example of a life consecrated to the highest intellectual aims. To the presence, the work, the example of such a master as Prof. Sylvester, the teachers of the Johns Hopkins University all owe, each in his own measure, guidance, help, inspiration; and in grateful recognition of all that he has done for them, and through them for the University, they wish for him a long and happy continuance of his work in his native land; for themselves the power of transmitting to others that reverence for the ideal which he has done so much to make the dominant characteristic of this University."

AN ascent of Ben Nevis was made on Monday by Mr. C. D. Cunningham, a member of the Alpine Club, accompanied by M. Emile Rey, a Swiss guide, and John Cameron, the well-known guide at Fort William. There were about six inches of snow on the ground from the commencement of the new road to the Red Burn. Here considerable difficulty was experienced in crossing the Burn and arriving on the top of the opposite bank, owing to the great quantity of snow which had drifted into the watercourse. From the well to the summit the ground, covered with deep snow, was hard frozen, making the task comparatively easy. Mr. Omond and his companions at the Observatory appeared in good health and spirits, and entertained the party in the most hospitable manner. The ascent occupied three hours thirty-five minutes, and the descent two hours.

THE estimates submitted to the Dominion Parliament include (says a Reuter's telegram from Ottawa) the sum of 25,000 dollars for the expenses connected with the meeting of the British Association at Montreal this year.

THE German Cholera Commission has sent a fifth report from Calcutta, dated January 5. Dr. Koch seems to have really discovered special cholera bacilli. The Commission was further occupied with the investigation into the causes of the great decrease in cholera mortality in Calcutta, where the percentage of deaths per thousand has diminished from ten to three. This diminution is attributed to the improvement of the water supply.

THE Nautical Meteorological Office of Sweden maintains at present nineteen stations at which meteorological observations are made on a large scale, twenty stations for measuring the fall of rain and snow, and sixteen hydrographical observatories. Weather journals were last year received from eleven men-of-war and fifteen merchantmen. The Meteorological Office in London

having requested that of Sweden to forward as complete journals as possible of the meteorological phenomena of the North Atlantic Ocean between August 1, 1882, and September 1, 1883, the Office has made a careful abstract of these journals for this purpose.

THE consistory of the Upsala University has voted a sum of about 200*l.* for the purchase of objects of natural history for the University collected by the *savants* of the *Vanadis* Expedition round the world, now taking place.

On January 14 a "green" moon was observed at Kalmar in Sweden. At about 5 p.m., just after the sun had set, leaving an intense purple glow on the sky—more intense than the late sun-glows—the moon came out of a layer of heavy clouds in the east. A few seconds after—the disk being then perfectly clear—a light haze gathered around it, partly veiling it, which immediately changed the bright silver colour to an emerald green. The phenomenon lasted for three minutes, when the disk again by degrees assumed its former brightness. A similar phenomenon was observed near Stockholm on January 17 at about 8 o'clock in the morning. It lasted about three minutes.

THE Council of the Royal Meteorological Society have arranged to hold, at 25, Great George Street, S.W., by permission of the President and Council of the Institution of Civil Engineers, on the evening of March 19 next, an Exhibition of Thermometers. The Committee will also be glad to show any new meteorological apparatus invented or first constructed since last March; as well as photographs and drawings possessing meteorological interest.

A SPECIAL meeting of the Committee of the Sunday Society was held on Monday afternoon, February 4, at 9, Conduit Street, W., Prof. W. H. Corfield, M.D., in the chair. The Honorary Secretary submitted a Report on the recent voting as to the future political action of the Society, from which it appeared that 391 had voted in favour of making the Sunday opening of museums a test question at elections of Members of Parliament, and that 470 voted against this proposal; 853 voted in favour of making the question the subject of an annual motion in the House of Commons, and only 11 voted against this proposal. Resolutions were subsequently passed pledging the Society to action in accordance with these results.

LIKE its better known namesake in the metropolis, the Royal Institution, Liverpool, has done much to popularise scientific knowledge during the present century. So far back as 1820 it first gave a permanent home to a scientific society in Liverpool, by admitting the Literary and Philosophical Society to share its roof, for the purpose, say the Minutes, "of extending the knowledge of arts and sciences." Since then the number of societies with scientific aims has steadily grown in Liverpool, and the number of members composing them to some extent increased as steadily. The accommodation of the Institution is found to be limited, and the idea of devoting the whole of the available space for the purpose of meetings is beginning to take definite shape, and was supported by Mr. Morton, F.G.S., in his presidential address last week. A very large part of the building is occupied by the museum, which was formerly the most important in Liverpool; for many years not less than 30,000 persons visited it on free days annually; this number was maintained up to 1868-69, when it all at once fell off; last year the number was only 4489, of whom only 1019 visited the natural history collections. This diminution of interest was coincident with the opening of the Free Public Museum. In 1817 the Institution disposed of the mammalia, reptiles, fishes, crustacea, polyzoa, and corals in the museum, and it is thought desirable that the remaining collections of interest and *local character* should be absorbed into the Corporation Museum. The Institution has schools which are in an exceedingly prosperous condition, and its library has a large collection of standard works in natural science.

IN a letter on the remarkable sunsets from Mr. S. E. Bishop, dated Honolulu, January 15, the writer mentions the important fact that the reddish haze was seen 4000 miles west of Panama on September 3 from the barque *Southard-Hurlburt.*

THE Worshipful Company of Clothworkers has been pleased to grant a donation of 21*l.* to the National Health Society, 44, Berners Street.

A PROPOSITION has been presented to the Municipal Council of Paris to give the name of Darwin to a new street about to be opened.

THE Hotel Dieu, Paris, having Gramme machines and steam-engine, the Administration of the Assistance Publique has decided to introduce experimentally the use of Edison incandescent lights in the halls inhabited by patients. The Hotel Dieu is the largest and the leading French hospital.

THE French Minister of Public Instruction will organise in Paris an exhibition of the objects which have been collected at Cape Horn by the *Romanche.* The collection is composed of 170 cases of valuable specimens of mineralogy, geology, and zoology, as well as living plants which will be acclimatised as far as possible in French forests.

THE International Association of Electricians, of which we have announced the creation in Paris, will hold its monthly sittings at the rooms of the Society of Geography. The first took place at the beginning of this month. The first part of the *Transactions* of the Association has reached us.

A NEW popular scientific paper has been published in Paris entitled *Le Mouvement Scientifique.*

THE Aristotelian Society for the Systematic Study of Philosophy will meet henceforth in the rooms of the Royal Asiatic Society, 22, Albemarle Street, W.

SHORTLY before sunset on Tuesday evening when the whole of the population of Ñotaresco, in the Abruzzo, had retired within doors on account of the intense cold, a shock of earthquake was felt, of such severity that the people rushed headlong into the streets and remained there until after midnight. The shock was also felt at Atri, Guilianova, Avellino, and Citta Sant' Angelo. A violent earthquake also occurred on the 10th inst. in the district of Birvari, Province of Bitlis, Turkey. A large number of houses and other buildings were thrown down.

MOUNT ETNA has, since Saturday, entered into an eruptive stage by throwing out ashes from the topmost crater. Strong earthquake shocks in the districts around the mountain preceded the outbreak.

AN unusually bright meteor was seen in Western Germany on January 28, about 7.30 p.m. At Barmen its motion seemed to be from east to west, while at Neuwied south to north was the direction. Its brilliancy is generally compared to that of the full moon.

AT the last meeting of the Berlin Anthropological Society, Prof. Nehring reported on the discovery of a cave near the village of Holzen (Brunswick), which is of special interest, inasmuch as there is strong evidence of cannibalism among the ancient cave men of that place, the first time that such evidence is forthcoming concerning the prehistoric inhabitants of what is now Germany. In Belgium and Spain similar evidence had been found, but had been dismissed as doubtful. The bone-remains of the Holzen cave are not completely calcined; at the

same time there is proof that the bones were opened to get at the marrow. But the strongest evidence of cannibalism was furnished by the arrangement in which the bones were found. Besides these bones and bone implements, roughly worked bronzes were found. At a lower level numerous lemming bones were found, which, with regard to the age of the cavern, seems to point to the Glacial epoch. In the debate following Prof. Nehring's report, Prof. Virchow raised some doubts regarding the cannibalism of the cave dwellers.

A MEETING of delegates of Natural History Societies in the east of Scotland (including the counties of Fife, Perth, Forfar, Kincardine, and Aberdeen) was held in the lecture-room of the Perthshire Natural History Museum, Perth, on Nebruary 9, to consider the question of federation alluded to in NATURE. The following societies were represented :—Aberdeen Natural History Society, Alford Field Club and Scientific Society, Arbroath Horticultural and Natural History Association, Dundee Naturalists' Society, Dundee Naturalists' Field Club, Kirkcaldy Naturalists' Society, Largo Field Naturalists' Society, Montrose Natural History and Antiquarian Society, and the Perthshire Society of Natural Science—being all but four of the Societies in the above mentioned counties. Two of these four societies considered that their objects did not quite entitle them to join the proposed federation, at least for the present; and from the other two no response had been received. After deliberation it was resolved to federate the societies under the title of "The East of Scotland Union of Naturalists' Societies." The objects of the Union are the promotion of good and systematic work by the various societies in it, and of friendly intercourse amongst their members; its affairs are to be conducted by a council of representative members, two being elected by each society. The president is to be a man of scientific eminence, connected with the district; and it is to hold an annual general meeting at the headquarters of the various societies in rotation, and other meetings in such places in the district as may be agreed on. The Union starts with a membership of about 1300. It was determined that the first general meeting should be held in Dundee on June 6 and 7 next. Dr. Buchanan White, F.L.S., was elected President, and Mr. F. W. Young, F.R.S.E., Hon. Secretary of the Dundee Naturalists' Society, was appointed Secretary.

WE learn from *Science* that Mr. Joseph Wharton of Philadelphia writes to the *Public Ledger* of that city (January 22) that he has found volcanic glassy dust in fresh, clean snow of recent fall. The snow, melted under cover in the porcelain vessel it was gathered in, showed at first no sediment; but after a time, and aided by a gentle rotatory movement which brought all to the deepest point, a slight deposit appeared. By pouring off most of the water, and evaporating the remainder, a little dry dust was obtained, which, even to the naked eye, showed, in the sunlight, tiny vitreous reflections. The dust weighed by estimate a hundredth of a grain, and showed under the microscope the characteristics of volcanic glass. It was partly irregular, flat, and blobby fragments, and partly filaments more or less contorted, which were sometimes attached together in little wisps, and were mostly sprinkled with minute glass particles. Under a knife-edge the filaments broke easily and cleanly. The irregular fragments were of various sizes and shapes, mostly transparent, but, even when examined by strong transmitted light, showing no trace of crystalline structure. Their diameter was about that of single filaments of silk. No crystalline particle of pyroxine, or black crumb of augite, such as observers have found elsewhere in similar dust, was present; nor did a strong magnet stir any particles of magnetic oxide of iron, though they also have been found in other volcanic dust. It may fairly be assumed that those heavier minerals, if at first mingled with the volcanic glass, had been already deposited during the long voyage through more than ten thousand miles of space and more than four months of time, while the tenuity of the intrinsically lighter glass threads (the Pele's hair of Mauna Loa) enabled them to float farther from the point of eruption.

"THE International Conference for fixing upon a universal prime meridian and a universal system of time has," *Science* states, "at length been called by the State Department to meet in Washington, Oct. 1. Diplomatic proceedings are always expected to go on with a certain dignified leisure; but the arrangements for the meeting of this conference have been delayed far beyond anything customary even in diplomacy. The act authorising the conference became a law in August, 1882. As there was some doubt whether there would be a sufficiently general response to the invitation to insure the success of the conference, a preliminary circular requesting the views of the various governments interested, and an expression of their willingness to enter the conference, was issued from the State Department toward the end of 1882. The responses were in some cases favourable, and in others negative or undecided. A desire was felt by the Europeans to have a preliminary discussion of the subject at the International Geodetic Conference at Rome in October, 1883. The feeling at this conference having shown that there would be little difficulty in the universal adoption of the Greenwich meridian, the final step of calling the conference was taken. Why so late a date was chosen we are not informed."

THE Magdeburg Wetter Verein has been transformed into a branch of the general German Meteorological Society, which is under the direction of Dr. Neumayer of Hamburg.

THE valuable ethnological collection made by Herr Zembsch at Apia, for many years German Consul-General at that place, has been purchased by the Ethnological Museum at Berlin. It consists of over 500 specimens.

THE additions to the Zoological Society's Gardens during the past week include a Chacma Baboon (*Cynocephalus porcarius* ♀) from South Africa, presented by Col. Gildea; a Macaque Monkey (*Macacus cynomolgus* ♂) a Black Kite (*Milvus migrans*) from India, presented by Mr. John M. Hagerman; a Common Hedgehog (*Erinaceus europæus*), British, presented by Mr. Archibald Aitchinson; a Bonnet Monkey (*Macacus sinicus*) from India, presented by Mr. J. Wilson; a Vulpine Phalanger (*Phalangista vulpina*) from Australia, presented by Capt. F. R. Slater; two Common Jackdaws (*Pica rustica*), British, presented by Master Harrott; a Chanting Hawk (*Melierax musicus*) from South Africa, a Partridge Bronze-winged Pigeon (*Geophaps scripta*) from New South Wales, purchased.

## OUR ASTRONOMICAL COLUMN

PONS' COMET.—It appears that this comet was sufficiently conspicuous to attract the attention of unscientific passengers on board one of our mail steamships in approaching Rio de Janeiro from the south on January 20, while it was an object of popular interest in Southern Italy towards the end of that month, according to the Naples correspondent of the *Times*. Observers in the other hemisphere may be able to follow it for several months longer; in the last week in June the theoretical intensity of light will be equal to that at the date of its discovery by Mr. Brooks.

This comet approaches the orbit of Venus within 0·076; that of Jupiter within 1·98; and that of Uranus within 1·17. The ascending node falls at a distance of 15·46. During the revolution 1812–1884, the calculations of MM. Schulhof and Bossert show that the approximate effect of planetary attraction upon the periodic time, at the instant of perihelion passage in the former year, has been as follows :—

| | Days |
|---|---|
| Comet accelerated by action of Jupiter ... ... | 446·49 |
| ,, ,, ,, Saturn ... ... | 13·96 |
| Comet retarded by action of Uranus ... ... | 13·48 |
| ,, ,, ,, Neptune ... ... | 1·48 |

Hence the period of revolution in 1812 has been shortened by perturbation to the extent of 445·49 days. The orbital velocity of the comet at perihelion is 29·2 miles in a second, at aphelion it is 3550 feet in the same time.

THE GLASGOW CATALOGUE OF STARS.—Prof. Grant has just issued the important catalogue of stars which has been for some time in active preparation at the Observatory of Glasgow, and towards the publication of which the Royal Society has largely contributed from the Government Grant Fund. Its appearance is too recent to allow of a description of the contents in the present column.

THE VARIABLE STAR U GEMINORUM.—Mr. G. Knott, writing from Cuckfield on the 4th inst., sends observations of a recent maximum of this star; his estimates are:—

| | h. m. | | | | h. m. | | |
|---|---|---|---|---|---|---|---|
| Jan. 24, | 8 10 | ... | 13·3 m. | Jan. 28, | 8 15 | ... | 9·95 m. |
| 26, | 9 50 | ... | 9·6 | 30, | 9 0 | ... | 11·4 |
| 27, | 8 53 | ... | 9·7 | Feb. 2, | 7 20 | ... | 13·9 |

Clouds prevented observation on January 25, but it is quite possible that the maximum may have been attained on that day, since in 1877 the star increased from 13·2 m. to 9·8 m. between February 20, 8h. 10m., and February 21, 10h. 30m. The last previous maximum observed by Mr. Knott fell on January 30, 1883, the date also assigned by the observations of M. Safarik (*Astron. Nach.* No. 2505).

The period which best represented the observations in the years immediately following the discovery of the star's variability by Mr. Hind (in December 1855) was 97 days, but there has been subsequently great irregularity, and according to Mr. Knott it has fluctuated between 71 and 126 days, though the values on the whole cluster about a mean of from 90 to 100 days; the limits of variation being about 14½ and 9¼ of Argelander's scale. These inferences are drawn from thirty-four maxima, observed partly by Mr. Knott and partly by Mr. Baxendell (see the *Observatory*, April, 1882).

THE LATE J. F. JULIUS SCHMIDT.—Practical astronomy has sustained a serious loss in the sudden death of the well-known Prof. Julius Schmidt, who has been for many years Director of the Observatory at Athens. According to a Reuter's telegram his funeral, which took place on the 8th inst., was of a public character, the King and Queen of Greece being present at the Observatory during the delivery of the funeral oration. A notice of Prof. Schmidt's long-continued astronomical labours must be deferred to another week.

## THE ROYAL SOCIETY OF EDINBURGH

AT the meeting of this Society on the 4th inst., the President, Lord Moncreiff, delivered an address on "The Past Hundred Years' History of the Society." Regarding this long interval, Lord Moncreiff said: "From the watch-tower of the Royal Society I can trace within the century a revolution more wonderful and more extensive than monarchs, or empires, or republics can display. Since this Society held its first meeting, how great to the community has been the fruit gathered from those branches of knowledge which it was incorporated to prosecute! During that interval, what has science not done for human comfort and happiness? What interest so great, what dwelling so humble, as not to have felt its beneficent influence? Since the invention of the art of printing, no such advance in material comfort, prosperity, and intelligence has ever been made within a similar period as this century has witnessed. Its triumphs have not been confined to the more abstruse fields of thought and study, but have come straight to the world of every-day life. One homely illustration meets me on the threshold of the opening night, and homely things go deep into the foundations of human life. I picture to myself our founders wending their way to the College Library, through close and wynd, in mid-winter 1783, while flickering oil lamps made the darkness visible without, and a detestable tallow candle made the student miserable within doors. Those who cannot recollect the universal reign of tallow candles and their sufferers, cannot appreciate how much the sum of human enjoyment has been enhanced, and the tranquillity of human temper increased, by the transmutation—partial, we must admit—of darkness into light. There has been, I believe, no more potent agent in humanising the denizens of our large cities than the flood of light which chemical science has in our day poured into their recesses. Prophets tell us that, before the end of the century which we now begin, gaslight will probably have followed the tallow candle into the same unlamented obscurity; but, even should this be so, history will carry to its credit the vast amount of public utility, and the many hours of useful employment or comfort in the factory, the study, or the sick-room, which this simple application of chemical science gained in its day for the nineteenth century. But the dispersion of material darkness is but a slender illustration of the triumphs of scientific discovery. Time and space are no longer the tyrants they were in 1783. I rather think that when our founders first met they could hardly hope to hear by post from London under ten days, as Palmer's mail-coaches had not begun to run until 1789. It would be an interesting inquiry, if my limits permitted, to trace the moral and social effects of the change from the days when a London letter took even three days to reach Edinburgh, and cost 13½d.—the pre-Macadamite days, when twenty miles a day was a fair posting rate on any roads but the main thoroughfares. Lord Cockburn lamented over the prospect of London being within fifteen hours of Edinburgh, as endangering the characteristics of our social community. His sagacity was not altogether at fault, but even that time has been reduced by a third, and I rather think we and the world are all the better for the change. But although larger victories were in store for the century, they came slowly. Both Boulton and James Watt were original members of the Royal Society, but it was more than thirty years before steam navigation became general, and more than fifty before the first passenger railway train ran in Scotland. No doubt, in 1791, Erasmus Darwin, in his 'Botanic Garden,' a poem too little read, had exclaimed in the well-known lines:—

> 'Soon shall thy arm, unconquered steam, afar,
> Drag the slow barge, and urge the flying car.'

Godwin, too, looked forward with confidence to the ultimate victories of steam. Now, the locomotive carries mankind to all ends of the earth; their sanguine suggestions have been all but realised. There has been during this interval a still more powerful magician at work. To this audience I need not dwell on the triumphs of the future ruler of the world of science—electricity. But one illustration I may be permitted. Franklin was one of the first of the non-resident members elected by the Royal Society of Edinburgh. How little he thought when, many years before, he drew the electric spark from the cloud, that, before 100 years had sped, his experiment, but slightly modified, might convey a message from a meeting of the Society in Edinburgh to one of its fellows in New York, and bring back an answer before the meeting separated. In slightly alluding to this scientific revolution, my object has been partly to illustrate the surroundings of 1783, and also to remind my hearers that, of all the changes the century has seen, far the most important and the deepest have been the work of science. Increased facilities for inter-communication carry with them a complete change in the economical and social condition of the communities they affect. New wants, new customs, new ambitions, new possibilities, follow in their train by the operation of inevitable laws. By this talisman we have seen, perhaps sometimes without due appreciation, many a social problem solved which had before seemed hopeless; and although in the process of transition some period of adaptation may be necessary, and some temporary hardships endured, the result in all cases must be beneficent, and is, at all events, beyond the power of lawgivers to control or to resist.

"The Edinburgh Royal Society sprung partly out of the example of the Royal Society of London. But its immediate antecedent was the Philosophical Society, which had been founded nearly fifty years before by the celebrated McLaurin, and contained many distinguished names. Lord Kames became its president, and raised it to considerable distinction, both in science and literature, although that vigorous and versatile thinker and writer did not live to witness the commencement of the new institution. Dr. Robertson's plan was to absorb this Society and all its members in a new institute, on the model of the Berlin Academy of Sciences, for the prosecution both of physical science and of literature. The charter, however, was not obtained without some controversy, for, even as Romulus and Remus quarrelled over the boundaries of unbuilt Rome, so did the Philosophical and the Antiquaries squabble over the charter of the Royal Society. The Antiquaries wanted a charter of their own; Dr. Robertson thought Scotland not wide enough for two such institutions; the feud ran high, and great was the "dust," as Prof. Dalzel calls it, which was raised by Lord Buchan on the occasion. Some notice of this dispute will be

found from the Antiquaries' side of the question in the recent life of Henry Erskine; and it is also alluded to in Mr. Cosmo Innes' work, where a letter is quoted from the energetic Professor of Greek couched in terms more forcible than philosophical. But it is certainly time to bury such feuds when they come to be a hundred years old. I find, from the minutes of the first meeting, that the Society was of opinion that the College Library was an inconvenient place for their usual meetings, and a committee was appointed to find one more suitable, apparently without success, for they continued to be held in the Library for twenty-three years, when the Society migrated to the Physicians' Hall in George Street in 1807. They afterwards purchased No. 40, George Street, in which the meetings were held until they obtained their present rooms in the Royal Institution. At a subsequent meeting, held on August 4, 1783, it was resolved that the Society should divide into two classes, which should meet and deliberate separately, to be called the Physical Class and the Literary Class, with separate office-bearers.

"The first president was Henry, Duke of Buccleuch, who had rendered great assistance in obtaining the charter. The vice-presidents were the Right Hon. Henry Dundas and Sir Thomas Miller, the Lord Justice-Clerk. I forbear to go over the names of what may be called the original members of the Society. I include in that term all who were elected within the first ten years. All the members of the Philosophical were assumed without ballot; the rest, to the number of more than 100, were elected by ballot, and a general invitation was made to the Lords of Session to join. These were the ordinary resident members. There was also a list of non-resident members, which comprised nearly as many. Of the ordinary resident members there is hardly a name which is not known—I might say conspicuous—in the annals of Scotland at that time. Twelve of the Lords of Session accepted the invitation, including the Lord President, the Lord Justice Clerk, and the Lord Chief Baron of the day; upwards of twenty professors, with Principal Robertson at their head; twenty-two members of the bar, including Sir Hay Campbell, the Lord Advocate, and of these at least fourteen rose afterwards to the bench. The medical contingent included Munro, Cullen, Gregory, and Home; and the non-resident list contained the names of the Duke of Buccleuch, the Earl of Morton, the Earl of Bute, the Earl of Selkirk, Lord Daer, James Steuart Mackenzie, the Lord Privy Seal, Sir George Clerk Maxwell of Penicuik, Sir James Hall of Dunglass, and many other familiar names. But I select from the list those of the members on whom fell the burden of the real work; and I venture to say that no city in Europe could have brought together a more distinguished circle. They were—Hay Cambell, Henry Dundas, Joseph Black, James Hutton, John Playfair, Adam Smith, William Robertson, Dugald Stewart, Adam Ferguson, Alexander Monro (*secundus*), James Gregory, Henry Mackenzie, Allan Maconochie, and William Miller of Glenlee. I ought to add to these Sir James Hall of Dunglass, and Sir George Clerk Maxwell of Penicuik, the last of whom died the first year. Some of these names are European; all are celebrated; and these were men who, for the most part, did not merely contribute the lustre of their names to the infant Association, but lent the practical vigour of their great intellectual power to aid in the first steps of its progress. And very soon the impress thus stamped on the Society began to establish its reputation in the world, and it took no undistinguished place among the learned societies of Europe. I find the names of Goethe and Buffon among the original foreign members; and although the events of the next twenty years interrupted our relations with the Continent, by the time the Society had completed the half-century there was scarcely a distinguished *savant* in Europe who had not joined, or been invited into, our ranks.

"In the Physical Class were four men who rose to great positions in the scientific world, and to whom the Society was greatly indebted for their general reputation, and for the vigour and efficiency with which their proceedings commenced. They were James Hutton, Joseph Black, John Playfair, and Dugald Stewart. Hutton and Black were then in the zenith of their fame, and have left a strong impress on the first years of our Society. I am desirous, in this review of the Society's early days, to revert with gratitude and respect to the memory of one whose labours on behalf of the Society were invaluable. Hutton was an observer and a thinker of remarkable originality and power. Black, again, was a Frenchman by birth, although his parents were British, and he was nearly related both to Adam Smith and to Adam Ferguson. He came to Scotland when he was about twelve years old, and, long before the institution of the Royal Society, had risen to the front rank of European chemists—his discoveries on pneumatic chemistry and latent heat having laid the foundation of much that is valuable in subsequent investigations, and opened a course of inquiry pursued with great ability in our own *Transactions* by Leslie, and Brewster, and Forbes." Lord Moncreiff having glanced at some peculiarities of the social meetings of those days between Black, Adam Smith, Hutton, and others, proceeded to speak of Playfair and Dugald Stewart, who by themselves could have raised to distinction any circle to which they belonged. "Both of them were men of great versatility, and, within the walls of the Royal Society, capable of filling a foremost place whether in the fields of abstract science or in those of literature or mental philosophy. Dugald Stewart's contributions to the *Transactions* are not so numerous as those of Playfair; but no man had more influence in moulding the tone and cast of thought prevalent among the cultivated class of his countrymen than that most popular and most eloquent instructor of youth. But no one can study these volumes of the *Transactions*, as I have done, without feeling that, for the first two decades of the existence of the Royal Society, Playfair was the soul and life of the institution. His versatility and power have impressed me exceedingly, high as was the estimate I had previously formed of him. Profound and transparently clear, whatever might be the topic, he bears about with him a far-reaching vigour which never flags. Whether it be the origin and investigation of porisms, or the astronomy of the Brahmins, or their trigonometrical calculations, or meteorological tables, or a double rainbow, nothing seems too great or too small for him.

"There are many curious and interesting by-paths, both of science and of literature, traversed in these earlier volumes. In 1787 Mr. George Wallace read a paper, which he did not incline to have printed in the *Transactions*, which I regret, for it related to a subject the interest of which has not ceased by the lapse of nearly a century. Its title was, 'On the Causes of the Disagreeableness and Coldness of the East Wind.' In the first volume of the *Transactions* a very singular problem was presented to the Society, through Mr. Adam Smith, along with other learned bodies in Europe, by a Hungarian nobleman, Count Windischgratz, and a prize was offered by him of 1000 ducats for the best solution of it, and 500 ducats for an approximation to a solution. It was a bold effort of philanthropy, for its object was the abolition of lawyers for the future. The problem was addressed to the learned of all nations. It was couched in Latin, but was in substance this:—'To find formulæ by which any person might bind himself, or transfer any property to another, from any motive, or under any conditions, the formulæ to be such as should fit every possible case, and be as free from doubt and as little liable to controversy as the terms used in mathematics.' I suppose that the prospect here held out of dispensing for the future with the least popular of the learned professions inclined the Society to entertain it favourably, for they proceeded to invite solutions of the problem, and three were received by them. In 1788 we find it recorded in the minutes that Mr. Commissioner Smith (for so the author of the 'Wealth of Nations' was designated) reported the opinion of the Committee that none of the three dissertations amounted to a solution, or an approximation to a solution, of that problem; but that one of these, with a certain motto, although neither a solution nor an approximation to a solution, was a work of great merit; and Mr. Fraser-Tytler was instructed to inform Count Windischgratz of their opinion. Whether this meritorious dissertation obtained the 500 ducats or not, we are not informed, but as lawyers continue to flourish, and legal terminology to produce disputes as prolifically as ever, it seems clear that the author had not earned them.

"Now that we have an Observatory on Ben Nevis, our successors at the end of the next century will know accurately the conditions of the climate under which the hundred years have been spent. There are, however, some details scattered over these volumes which are sufficiently interesting, although whether they show any material alteration in our seasons may be doubtful. The only cheering fact which they disclose is that the first set of returns is decidedly the most discouraging, and certainly does not support the idea that the mean temperature in the olden time was higher than it is now. There are two sets of returns printed in the first volume of the *Transactions*—one kept at Branxholm from 1773 to 1783, communicated by the Duke of Buccleuch, who was the

first president of the Society; and the second by Mr. Macgowan, kept at Hawkhill, near Edinburgh, from 1770 to 1776. In the first, the mean temperature of the ten years is 44°; in the second, 45°—not a very genial retrospect. Things must have been somewhat discouraging for the farmers in 1782, for a paper is noticed in the second volume of the *Transactions*, by Dr. Roebuck, of Sheffield, who was the manager of the Carron Iron Works, recommending farmers not to cut their corn green in October, although there was ice three-quarters of an inch thick at Borrowstounness, because corn would fill at a temperature of 43°. Things looked brighter from 1794 to 1799, for which years we have results furnished by Playfair. For the first three years—1794, 1795, and 1796—the mean temperature was 48°; and that although 1795 was one of the most severe winters on record, the thermometer having stood frequently several degrees below zero, and a continuous frost having lasted for 53 days. The mean temperature in 1794, however, was 50°. The account of the great frost of 1795, which is given in the *Transactions*, is well worth referring to. In the next three years the mean temperature was 48°, that of 1798 being 49°·28. Of this year (1798) Playfair says that the climate of this part of the island hardly admits of a finer season. No tables were furnished to the Society, in continuation of those of Prof. Playfair, until 1830, when fortunately Dr. Barnes of Carlisle communicated to the Society a series of meteorological tables kept at Carlisle for the first twenty-four years of the century. The results seem mainly to concur with those of Prof. Playfair—the mean temperature for the twenty-four years being 47°·4547, being 3° higher than the average of the ten years from 1773 to 1783 at Branxholm, and 2° higher than the mean temperature of the seven years from 1770 to 1776 at Hawkhill. The highest temperature I have noted in these returns is that of May 1807, when the thermometer stood at 85° at Carlisle, and the next, that on the 5th of August, 1770, when the thermometer at Hawkhill was at 81°. The two years of the century in which the mean temperature was the highest were 1811 and 1822, in both of which years it was 49°.

"Of the purely scientific part of the Royal Society's work for the first fifteen years of its labours, while Hutton and Black and Playfair and Stewart were in full vigour, it is not too much to say it was brilliant—full of interest, full of power, and full of enthusiasm. The first great founders of course gradually waned, and all such associations are necessarily subjected to alterations of the tide, but as the tale goes on the mathematical papers begin to bear the names of John Leslie and William Wallace. We encounter Walter Scott in 1800, in 1808 the name of David Brewster, and in 1811 that of Sir Thomas Macdougall Brisbane, whose names adorned and whose labours were in the future the prop and stay of the Society. Of Scott I need not speak; but of the services rendered by Brewster it is impossible to express myself too strongly. He, too, like Playfair, had a mind of rare versatility. He could observe, as well as draw from his own resources. He could reason as well as describe. He could build a framework of sound deduction from the most unpromising hypothesis, and work out with unflagging spirit the thread of demonstration, however slender. He was the most prolific contributor of his day; nor do I think that any one but himself in these times could have kept the fire lighted by Hutton and Playfair burning so brilliantly. For it is not to be disguised that in the heat of the Continental struggle an air of languor creeps over the proceedings. The joyous enthusiasm of 1783 refuses to be invoked, and is elicited in vain. Nor is it wonderful. When the Gauls were so nearly at our gates, the safety of our own commonwealth was comparatively our only care. But when 1815 had arrived, and men's minds, set free from the long anxiety, had again tranquillity to cultivate the arts of peace, the energy of the rebound was great, and the history of British science has been one continued triumph ever since. By the exertions of Brewster and Brisbane, and many other associates, our Society again began to flourish, both leading and following the course of discovery as the stream flowed on. Both of these men continued to be the pride and ornament of the Society long after the expiration of the half-century which I have assigned to myself as my limit, for Thomas Brisbane succeeded Sir Walter Scott as president in 1832, and survived until 1860. Long before that a new generation had surrounded the veteran philosophers, and their destiny has been to recount and carry forward discoveries of which even Brewster and Brisbane hardly dreamt.

"Enough for the present of this retrospect, and the slender tribute I have attempted to pay to the memory and labours of a masculine and powerful generation. That we have built on their discoveries and learnt even by their errors is quite true; for the history of the second half of the century exhibits science far in advance of 1783, and even of 1833. In 1783 geology was in its infancy; palæontology was all but unknown. Cuvier was only then commencing his pursuits in comparative anatomy, which were to end in reproducing the forms of extinct life. The Glacial epoch had not then been elucidated by the research and genius of Forbes and Agassiz, and the dynamic theory of heat was still unproclaimed. The wonders of the photographic art were unknown even in 1833, for Talbot and Daguerre did not come on the scene for several years afterwards. In 1833 the apostle and disciples of evolution had not broken ground on that vast field of inquiry. Spectrum analysis and the marvellous results which it has already furnished and those which it promises have in our day only heralded the advent of a new science. But however far in advance of the founders of the Royal Society the current philosopher may be, there was a robustness and characteristic individuality about the great men of that generation which we may not hope to see replaced. We may assume—indeed, we hope—that the close of the next century will find the progress of knowledge as far advanced beyond its present limits as we think that the science of to-day is beyond the point reached a century ago. We may be assured that before that time arrives many surmises, still in the region of hypothesis, will have become certainties, and that many supposed certainties will have turned out fallacies. Many errors will have been corrected, many dogmas discredited, many theories confirmed or refuted, at the bar of ascertained fact, as those of 1783 have been. Yet even then will our successors, I trust, as we do now, stand reverently before the memory of our founders. Happy is the institution which can show such a muster-roll, and happy the country which can boast such sons. I take leave of my theme with the fervent hope and firm conviction that in the century which we now inaugurate the Royal Society will continue with success the noble task to which by its charter it is devoted, of investigating the hidden treasures of nature and appropriating them to the benefit and happiness of mankind."

## *INSTINCT*

### I. *Is there a Science of Comparative Psychology?*

"IN the family of the sciences Comparative Psychology may claim nearest kinship with Comparative Anatomy; for just as the latter aims at a scientific comparison of the bodily structures of organisms, so the former aims at a similar comparison of their mental structures." These words form the opening sentence of Mr. G. J. Romanes' Introduction to his recently published volume on "Mental Evolution in Animals," and in a footnote he is careful to remind us that the phrase "mental structures" is used in a metaphorical sense. Let us consider how far a comparison of the mental structures of animals, even in a metaphorical sense, is possible.

Our knowledge of mind is either direct or inferential: direct on the part of each individual so far as his own individual mind is concerned; inferential so far as the minds of others are concerned. For it is a law of our being that mind cannot come into direct contact with mind. This fact—that the mental processes of our neighbours can never come within the sphere of our objective knowledge—has long been recognised (see *ex. gra.* Berkeley, "Princ. Hum. Know.," §§ 27 and 145; Kant as quoted in F. Pollock's "Spinoza," p. 177); and the late Prof. Clifford (see "Lectures and Essays," vol. ii. p. 72) coined the exceedingly convenient term *ejective* as descriptive of that class of phenomena which belong neither to the subjective nor to the objective category. My neighbour's mind is not and never can be an object; it is an eject, an image of my own mind thrown out from myself. Into every human being that I meet I breathe this subtle breath; and that man becomes for me a living soul.

Our knowledge of mind is therefore partly subjective, partly ejective. Now it is perfectly obvious that, were I an isolated unit, shut off from all communication with my fellows, no science of psychology would be possible for me. I might by the analysis of my own mental processes arrive at certain conclusions with regard to my own states of consciousness; I might reach some sort of knowledge of the working of my own mind. But this would not be a science of mind. A science of mind only becomes possible when I am able to compare my own conclusions with those which my neighbours have reached in a similar manner. By means of language human beings can communicate to each

other the results which each has obtained; and each human being is able *to submit these results to the test of subjective verification*. For human beings therefore a *science* of psychology is possible just in so far as the results obtained indirectly are capable of direct *verification*.

One of the most remarkable results of modern scientific investigation is the establishment of a more or less definite parallelism between the phenomena of ejective psychology (thus capable of subjective verification) and certain objective phenomena of physiology—a parallelism of psychosis and neurosis. But these phenomena of physiology are not restricted to the human subject; and we therefore have grounds for believing that running parallel to the neuroses of animals there are certain psychoses. And it would seem at first sight possible that corresponding to a science of comparative neurosis we might have a science of comparative psychosis. We must remember, however, that it is only on the lower mental levels, so to speak, that we know anything approaching to definiteness with regard to the parallelism of neurosis and psychosis. All, therefore, that, as scientific investigators, we seem to have any grounds for inferring is that accompanying the neuroses of animals there are in all probability some kind of psychoses. We may speculate as to the character of these psychoses—and in the case of the higher mammalia our speculations are probably by no means worthless—but we cannot construct a comparative *science* of these psychoses because the results we obtain ejectively are incapable of direct verification. As a speculation modern constructive psychology has its value—like other speculations it may give direction to our scientific investigations—but let us not forget that the invaluable process of verification is, from the nature of the case, impossible.

To sum up. All our knowledge of minds other than our own is ejective; but in the case of human psychology the results reached ejectively may be verified subjectively. Animal minds are also ejective; they are more or less distorted images of our own minds. But such is the extraordinary complexity of the human mind—a complexity largely due to the use of language—that we may well suppose that any conception we can form of animal consciousness is exceedingly far from being a true conception. The results of comparative psychology—the science which has for its object the comparative study of these distorted images of our own mental processes—are incapable of verification. These are the facts which have to be taken into consideration when we seek an answer to the question "Is there a science of comparative psychology?" Notwithstanding that it has won for itself a more or less recognised place among the sciences, I venture to submit that our answer to this question should be an emphatic negative.

It must be noted, however, that I here mean by psychology the science which deals with subject and eject. If we include under psychology the science which deals with the "perpetual adjustments of special inner actions to special outer actions which accompanies increasing evolution of the nervous system," or that to which Mr. Herbert Spencer gives the name *objective* psychology ("Prin. Psychol.," vol. i. p. 142), our answer will of course be different. Objective psychology, or the comparative physiology of the nervous system *plus* a comparative study of the corresponding adjustive actions, has every right to be termed a science because the results obtained admit of verification. And it is a science in which Ferrier, Hitzig, Romanes, and others have done good work.

## 2. *The Place of Consciousness*

There would seem to be four hypotheses with regard to the place of consciousness in the animal world.

1. That according to which consciousness is a motive power (Free Will).

2. That according to which consciousness is altogether absent (Automatism).

3. That according to which consciousness is a product (Conscious Automatism).

4. That according to which consciousness is a guide (Determinism).

1. *Free Will.*—By free will I here mean the power of initiating actions by the mere volition of the self-conscious *Ego*. The exercise of free will involves an interference *ab extrâ* with the normal working of the nervous system.

This is not the place for a discussion of free will and determinism. That battle must be fought out within the domain of human psychology. From its bearing on the question of animal consciousness, however, I may be permitted to say a few words on the subject.

The answer which the ordinary believer in free will gives to the determinist is contained in three words—*I can choose*—and he thinks that there is an end of the matter. But the real point at issue lies deeper down, and is involved in the question—*What am I?* Let us hear the answer which the determinist gives to this question. I am, he replies, the sum of my states of consciousness at any moment. Apart from the stream of my mental states I, as a self-conscious individual, have no existence. This stream of conscious states or psychoses I believe to be the subjective aspect of a stream of nervous states or neuroses. And this stream is rigidly subject to law. But if these states of mind—under which head must be included states of definite consciousness, states of sub-consciousness, and states of submerged consciousness—if these states of mind, I say, constitute *me*, then, since these states of mind determine those which follow, these following states, and the actions which accompany them, are determined by me. But at the same time they are part of an orderly sequence subject to law. The moment I identify myself with my states of mind I begin to see clearly that free will in the common-sense acceptation of the term—that is, a sense of individual choice—is perfectly compatible with the doctrine of determinism—that my mind is completely subject to law. The sense of choice I undoubtedly possess is due to the temporary equilibrium of motives, and the eventual prevalence of one set of motives over another set of motives. The freedom which every man is conscious of possessing is freedom to act in accordance with his own character.

"Freedom," says Kant, "is such a property of the will as enables living agents to originate events independently of foreign determining causes." This at first sight seems utterly opposed to determinism. And yet it contains a central core of truth which every determinist will accept. No determinist can deny that every human being carries about with him a special something, peculiar to himself, which is a most important factor—constituted as we are, the most important factor—in determining his choice in any act of volition. This special something we call, ejectively, his character, and, objectively, his organisation. Men are not like inorganic clouds at the mercy of external forces, but contain the springs of action in themselves. The brain is not merely a mass of inert matter; but a mass of matter cunningly organised, in which is locked up a vast store of potential energy. The organism is, moreover, a *variable* piece of mechanism. Hence at different times it reacts differently under the influence of the same stimulus. And this difference of reaction helps to fix the idea that the will is absolutely free. On a certain occasion we acted in a certain way. We see on reflection that our action was not the best. On a similar occasion afterwards we act differently. And we then imagine that we could have acted differently in the first instance. But it is clear that the two cases are not alike. Reflection has altered one of the determinants of action, the character. The character having changed, the action is different. Such a definition as Kant's—the essential truth of which I take to be that a man's actions are the outcome of his character—is as valuable to the determinist as to any one else. At the same time "it is inconceivable," as Chaldai Creskas said long ago (*circa* 1410), "that two men, being themselves of like temper and character, and having before them like objects of choice in like circumstances, should choose differently" (quoted from F. Pollock's "Spinoza," p. 96).

Determinism simply comes to this—that both on the objective side and on the subjective side our actions are determined by law. On the one hand a perfect knowledge of the organism *plus* a perfect knowledge of any stimulus and the surrounding conditions would enable us to say how the organism would act under that stimulus. On the other hand a perfect knowledge of the character *plus* a perfect knowledge of any motive and the circumstances of the case would enable us to say what feelings would result (the actions being the objective side of the feelings). If by free will it is meant that our actions are the outcome of the play of a motive-stimulus on our character-organisation, then free will and determinism are at one.

But this is not what is meant by those who maintain the doctrine of free will. What is meant by them is this—that presiding alike over our thoughts and actions, initiating, guiding, and inhibiting, there is a certain "masterful entity," the self-conscious *Ego*. This *Ego*, though in no wise connected with our bodily organisation, has nevertheless the power of interfering with the action of that organisation. And it is absolutely free, utterly unfettered

by law. This doctrine I reject: not because I am in a position to disprove it, but because I see no reason for accepting it. And rejecting this doctrine in the sphere of the human mind, I feel bound to reject it in the sphere of animal intelligence. But I am not blind to the fact that many of my neighbours do not reject it in the sphere of the human mind. To them two courses are open: either to extend it into the sphere of animal intelligence, or not so to extend it. If they do so extend it, they thereby render the study of animal intelligence incapable of scientific treatment, even from the objective standpoint, by the introduction of a factor not subject to law. If they do not so extend it, they must accept one of the three views next to be considered.

2. *Automatism.*—Very little space need be devoted to a doctrine that very few believe. Those who accept the doctrine of the parallelism (or identity) of neurosis and psychosis and add to this a belief in evolution are logically bound to accept the corollary that the neuroses of animals are accompanied by some kind of psychoses which more or less dimly foreshadow our own psychoses. Those, however, who reject the hypothesis of evolution, or at least deny its application to the mind of man, and who believe in the doctrine of free will as restricted to the human being, will, not improbably, accept the doctrine of automatism in animals. In any case it is a theory upon which the study of organic processes, reflex, instinctive, and intelligent (or selective), admits of scientific treatment. It is indeed "objective psychology" *plus* the dogmatic assertion that consciousness is absent.

3. *Conscious Automatism.*—"Materialism," says Mr. Romanes, "is logically bound to argue in this way: We cannot conceive of a conscious idea, or mental change, as in any way affecting the course of a cerebral reflex, or material change; while, on the other hand, our knowledge of the conservation of energy teaches us as an axiom that the cerebral changes must determine each other in their sequence as in a continuous series. Nowhere can we suppose the physical process to be interrupted or diverted by the psychical process; and therefore we must conclude that thought and volition really play no part in determining action. Thoughts and feelings are but indices which show in the mirror of the mind certain changes that are proceeding in the matter of the brain, and are as inefficient in influencing those changes as the shadow of a cloud is powerless to direct the movements of that of which it is the shadow. . . . This is opposed to common sense, because we all feel it is practically impossible to believe that the world would now have been exactly what it is even if consciousness, thought, and volition had never appeared upon the scene—that railway trains would have been running filled with mindless passengers, or that telephones would have been invented by brains that could not think to speak to ears that could not hear" (*Nineteenth Century*, December, 1882, p. 879). How far the materialist—the logical results of whose doctrine are apt to be forced on him from all sides—is ready to accept this particular logical result I leave it for him to say. It is at any rate a possible view, and, like that of unconscious automatism, is one upon which a scientific treatment of organic processes is admissible.

4 *Determinism.*—This view has already been incidentally given under the heading of the directly opposed doctrine of free will. It is the doctrine of the parallelism (or identity) of neuroses and psychoses, which, both in their subjective and objective aspects, are rigidly law-bound. Determinism may be treated either from the philosophical or from the scientific standpoint. From the point of view of the man of science we may say that consciousness is a *guide* to action and has been a guide in evolution; that during the process of evolution there gradually emerged something distantly related to what we know in ourselves as consciousness, which at a very early stage of evolution became, so to speak, polarised into pleasurable and painful; that those actions which were associated with pleasurable feelings were more frequently performed than those associated with painful feelings; that those organisms in which there was an association between right action and pleasurable feelings would stand a better chance of survival than those in which the association was between wrong actions and pleasurable feelings; and that finally those organisms in which conscious adjustments of all orders were more perfectly developed would be the winners in life's race. Some such deductions as these would seem to be admissible on the hypothesis of evolution. With such questions as How have psychoses become associated with neuroses? or Why have psychoses been associated with neuroses? or How can psychosis exercise a guiding influence on neurosis?—with such questions as these the man of science, as such, has nothing to do. These are questions for the philosopher, and this is, therefore, not the place to discuss them. Suffice it to say that we must either accept some such view as that advocated by Clifford in his masterly essay "On the Nature of Things in Themselves" ("Lectures and Essays," vol. ii. p. 71) or be content to confess our ignorance.

Upon this view of the place of consciousness in the animal kingdom, the study of organic processes, reflex, instinctive, and intelligent (or selective), admits of scientific treatment. A science of "objective psychology" is possible for us; and a science of ejective psychology is also possible, but not for us.

### 3. *The Lapse of Consciousness*

One of the most surely established inductions of psychology is this: that the more frequently an action is performed the more perfectly automatic does it become—the more does it tend to pass into stereotyped reflex action. Actions which are at first performed with that definite consciousness implied in the term close attention can, after frequent repetition, be performed almost, if not altogether, without even indefinite consciousness. It would seem that after the definite establishment of the nerve connections necessary for the performance of certain actions or sets of actions the guiding influence of consciousness might be withdrawn.

This principle is too well known to require illustration here. I shall therefore content myself with drawing attention to one or two of its corollaries.

1. Since the same action or set of actions may be performed with full consciousness—a consciousness of the end in view, and of the means necessary to that end—with indefinite consciousness, or with a vanishing amount of consciousness, it is impossible for me to say what amount of consciousness, if any, an action performed by my neighbour involves. Again and again we see our neighbours perform most complicated actions—such as winding up their watches—with so little consciousness as to leave no trace upon the memory. Abernethy quotes a case of a lawyer writing out an important opinion in his sleep. Still more impossible is it for me to say what amount of consciousness, if any, an action performed by one of my dumb companions involves. Decapitated frogs—in which we have some grounds for believing that consciousness is absent—perform a number of seemingly purposive actions.

2. Since those actions which are frequently and persistently performed by the individual have a tendency to pass into the automatic and unconscious stage, it would seem highly probable that those actions which have been performed not only by the individual but by a long line of ancestors whose organisation he inherits are, or very soon become, completely, or in a very high degree, automatic and unconscious. Who can say what amount of consciousness, if any, is involved in the actions of newly-born piglings or newly-hatched chicks?

3. It would therefore seem difficult or impossible to disprove the hypothesis that all truly instinctive actions—in so far as they are not modified (as they so often are modified) by a little dose of reason—are automatic and unconscious. I do not mean to maintain that hypothesis. But I say that, having regard to the known phenomenon of the lapse of consciousness, I do not see how that hypothesis could be disproved.

### 4. *The Psychological Definition of Instinct*

"Instinct," says Mr. Romanes in his recently published "Mental Evolution in Animals" (p. 159), repeating the definition given in "Animal Intelligence" (p. 17), "Instinct is reflex action into which there is imported the element of consciousness. The term is therefore a generic one, comprising all those faculties of mind which are concerned in conscious and adaptive action, antecedent to individual experience, without necessary knowledge of the relation between means employed and ends attained, but similarly performed under similar and frequently recurring circumstances by all individuals of the same species."

To such a psychological definition of instinct there seem to me to be two grave objections. First, there is the general objection, indicated in the first section, arising out of the ejective nature of our knowledge of animal consciousness. Secondly, there is the special objection raised under the head of "The Lapse of Consciousness." These objections have not escaped Mr. Romanes' notice, but I think he underestimates them. "No doubt," he says ("Ment. Evol.," p. 160), "it is often difficult, or even

impossible, to decide whether or not a given action implies the presence of the mind-element—*i.e.* conscious as distinguished from unconscious adaptation; but this is altogether a separate matter, and has nothing to do with the question of defining instinct in a manner which shall be formally exclusive, on the one hand of reflex action, and on the other of reason." But I venture to think that the difficulties of application are from the very nature of the case insuperable, and that the definition is therefore, whatever its logical value, practically of little service.

Again, on p. 17 of his recent volume, Mr. Romanes tells us that "the only test [of the conscious choice element] we have is to ask whether the adjustments displayed are invariably the same under the same circumstances of stimulation. The only distinction between adjustive movements due to reflex action, and adjustive movements accompanied by mental perception, consists in the former depending on inherited mechanisms within the nervous system being so constructed as to effect *particular* adjustive movements in response to *particular* stimulations, while the latter are independent of any such inherited adjustment of special mechanisms to the exigencies of special circumstances." And a little further on (p. 18) he says, "It is enough to point to the variable and incalculable character of mental adjustments as distinguished from the constant and foreseeable character of reflex adjustments." All which may be very true. But it seems to cut away the ground from under his definition of instinct. For surely what he says here of reflex actions is also true of instinctive actions. Surely instinctive actions "depend on inherited mechanisms within the nervous system being so constructed as to effect *particular* adjustive movements in response to *particular* stimulations." Surely we may also point to the "constant and foreseeable character of *instinctive* adjustments."

But though an instinctive action may involve no consciousness in the individual, it may have involved consciousness, during the process of its evolution, in the ancestors of the individual. In this way, perhaps, we may admit consciousness into our definition of instinct. But if we hark back to ancestors in one case, we may fairly do so in another. And since the secondary instincts of the individual involved intelligence in his ancestors, we must import not only consciousness but intelligence into our definition of instinct. If we admit lapsed consciousness, why not admit lapsed intelligence? Our definition will then become: Instinct is reflex action into which is imported (ancestrally) the elements of consciousness and intelligence. In which case instinct and reason run together.

It seems to me, therefore, that the psychological definition of instinct lacks that definiteness of application which is not merely desirable but essential. If I might be permitted to paraphrase Mr. Romanes I would say, "I am persuaded that if we are to have any approach to definiteness in the terms which we employ—not to say to clearness in our ideas concerning the things of which we speak—it is" *not* "desirable to restrict the word instinct to mental as distinguished from non-mental activity." And this just because it is so "difficult, or even impossible, to decide whether or not" instinctive actions "imply the presence of the mind-element—*i.e.* conscious as distinguished from unconscious adaptation."

### 5.—*A Physiological Definition of Instinct*

"Instinctive actions are actions which, owing to their frequent repetition, become so habitual in the course of generations that all the individuals of the same species automatically perform the same actions under the stimulus supplied by the same appropriate circumstances." This physiological definition of instinct, which is incidentally given by Mr. Romanes ("Animal Intelligence," pp. 16–17), is, if I mistake not, of more practical and scientific value than the psychological definition which immediately follows, and which introduces "the element of consciousness" and "faculties of mind."

Were it impossible to define instinct in such a manner as to be formally exclusive, on the one hand, of reflex action, and, on the other, of intelligent (or selective) action, without having recourse to the associated phenomena of consciousness, then it might be advisable to introduce consciousness into our definitions for the sake of giving them a logical status. And Mr. Herbert Spencer seems to see this difficulty when he defines *or describes* instinct as compound reflex action. But, though reflex action shades into instinctive action, and instinctive action (as seen in the phenomena described by Mr. Romanes, under the heading "The Plasticity of Instinct") into intelligent action, still some such definitions as the following would seem sufficiently to answer to the demand for formal exclusiveness:—

1. *Reflex Actions* are actions taking place in, or performed by, an individual in virtue of his possession of a general type of nervous organisation.

2. *Instinctive Actions* are actions performed by the individual in virtue of his possession of a special type of nervous organisation, that is, a type of organisation common to his species.

3. *Intelligent (or Selective) Actions* are actions performed by an individual in virtue of his possession of an individual nervous organisation, that is, an organisation special to himself.

If we call the foundation type of nervous organisation (in the mammalia, for example) $a$, the special modification of that type (in all dogs, for example) $b$, and the individual modification developed in some individual (say Dr. Huggins's "Kepler") $c$; then reflex actions are the outcome of $a$, instinctive actions the outcome of $a + b$, and selective or intelligent actions the outcome of $a + b + c$.

That there are difficulties in the application of these definitions to special cases I readily admit, but I venture to submit that they are by no means of so grave a nature as those involved in the psychological definitions advocated by Mr. Romanes.

I need not say here that such definitions do not by any means imply the absence of consciousness, since I have devoted a special section to *The Place of Consciousness* with the special object of showing that the doctrine of determinism, which I accept, maintains the parallelism or identity of psychosis and neurosis.

### 6.—*The Origin and Development of Instincts*

This article has already exceeded the length to which it was intended to run. On this head, therefore, I must be brief. The problem of the origin and development of instincts comes to this—How has it come about that certain nervous structures, and the actions which are their external and obvious manifestations, are developed in all the members of a certain species? It is clear that such a development of certain structures and their corresponding actions in all the individuals of a particular species must answer to a widely felt need. The actions answer to circumstances of frequent occurrence in the life-history of the species, just as intelligent actions "answer to circumstances of comparatively rare occurrence in the life-history of the species" ("An. In.," p. 17). The question is—How far is the equilibration direct, *i.e.* by adaptation, and how far is it indirect, *i.e.* by natural selection? To discuss this question would require a separate article. I content myself with giving two quotations, the former from Mr. Darwin, the latter from Mr. Spencer. "I believe that most instincts are the accumulated result, through natural selection, of slight and profitable modifications of other instincts, which modifications I look at as due to the same causes which produce variations in corporeal structures. . . . . But in the case of the many instincts which, as I believe, have not at all originated in hereditary habit, I do not doubt that they have been strengthened and perfected by habit; just in the same manner as we may select corporeal structures conducing to fleetness of pace, but likewise improve this quality by training in each generation" (quoted "Ment. Ev. in Ans.," p. 264). So far Mr. Darwin. Mr. Spencer says: "The equilibration of organisms that are comparatively passive is necessarily effected indirectly by the action of incident forces on the species as a whole. But along with the gradual evolution of organisms having some activity, there grows up a kind of equilibration that is relatively direct. In proportion as the activity increases, direct equilibration plays a more important part. Until, when the nervo-muscular apparatus becomes greatly developed, and the power of varying the actions to fit the varying requirements becomes considerable, the share taken by direct equilibration rises into co-ordinate importance" ("Princ. Biol.," vol. p. 468). It seems to me that we have here substantial agreement as to the part played by indirect equilibration in laying the foundation, and the part played by direct equilibration in perfecting the superstructure. (I venture to think that Mr. Romanes somewhat mistakes Mr. Spencer's position with regard to the "very subordinate importance of natural selection as an evolving source of instinct," and with regard to the question of "lapsed intelligence.")

### 7. *Conclusion*

One or two words in conclusion by way of summary.

1. While fully admitting the great interest that attaches to the study of the inferred mental faculties of the higher brutes, I believe that, from the ejective nature of the animal mind and the

necessary absence of verification, no science of comparative psychology, except such as is restricted to "objective psychology," is possible.

2. Of the four views of the place of consciousness in the animal world only one—that of *free will*—renders the study of the actions of animals incapable of scientific treatment. Of the other three I believe *determinism* to be the most satisfactory. According to this view both neuroses and psychoses are subject to law. But from our necessarily ejective knowledge of psychoses, we are forced to confine our attention (from the scientific point of view) to the objective phenomena of neurosis, especially as manifested in conduct. Of the psychoses we can know nothing with certainty; of the neuroses we may learn a little; of conduct we may learn much.

3. From the principle of the lapse of consciousness certain corollaries may be drawn—(*a*) that it is difficult or impossible to say what amount of consciousness, if any, an action performed by my neighbour involves; (*b*) that it would seem probable that the lapse of consciousness in the individual is paralleled by a lapse of consciousness in the species; and (*c*) that the hypothesis that instinctive actions are unconscious is incapable of disproof.

4. On the general grounds given in 1, and on the special grounds given in 3, I see grave difficulties in accepting the psychological theory of instinct—that instinct is reflex action into which is imported the element of consciousness.

5. In accordance with the principle thus advocated a physiological definition of instinct must be sought. Some such definition as this may be proposed: *Instinctive actions* are actions performed by the individual in virtue of his possession of a special type of nervous organisation, that is, a type of organisation common to his species.

6. The question of the origin and development of instincts thus becomes a question as to how this special type of structure has been evolved. It takes its place as part of the general question of the evolution of structures—the actions being the external manifestations of internal structures. To the question as to the relative importance of direct and indirect equilibration I could give no definite answer within the limits of this article, and therefore gave quotations from Darwin and Herbert Spencer.

C. LLOYD MORGAN

## *A NEW OBSERVATORY FOR PARIS*

THE last number of the *Comptes Rendus* of the Paris Academy of Sciences contains a memoir by Admiral Mouchez, urging the necessity of removing to a separate establishment beyond the city the chief departments of the Paris Observatory. When the building was originally erected by Perrault about a mile to the south of the Luxembourg, the city scarcely reached beyond that point. But since then it has spread in every direction, completely surrounding the Observatory with lofty edifices, and charging the atmosphere with all sorts of gases, smoke, and other impurities. These altered conditions are all the more injurious that, thanks to the progress of astronomical studies, the power and accuracy of the instruments have to be continually increased, while a clear and still atmosphere is more than ever needed for taking observations. The vicinity of the Catacombs and of busy streets has also rendered the ground less firm than formerly.

In 1854, and again in 1868, these adverse conditions were brought before the Government, and discussed in the Academy. After a careful study of the situation, the Commission appointed by the Academy to inquire into the matter unanimously reported in 1869 in favour of a branch establishment outside of Paris; but this suggestion, although fully approved of by the Academy, was for various reasons allowed to fall into abeyance.

Since then the evils complained of have been aggravated, in spite of all the improvements introduced for the purpose of modifying them. Hence it becomes more than ever indispensable to carry out the project forthwith, if the Observatory wishes to maintain its efficiency and keep pace with similar establishments abroad. The most serious obstacles to its legitimate development are the disturbed and clouded state of the atmosphere in the centre of a large city, the constant vibrations of the ground, and the impossibility of accommodating the astronomers in the building, as is done in all foreign observatories. Hence arises an insurmountable obstacle to the proper organisation of the night service, while extreme difficulty is felt in improving the existing plant and obtaining other much needed instruments, for which no suitable position can be found.

Merely to erect the long-contemplated tower and cupola of the great telescope there would be required a Government grant of from 20,000*l.* to 24,000*l.*, besides at least an equal sum to prevent the erection of lofty houses in front of the new grounds and to purchase the instruments still needed. But even were such grants obtained, the Observatory would continue to labour under the serious inconveniences above described. Without, however, imposing such a burden on the State, the difficulty might be met, and the old historical edifice of Louis XIV. preserved, by erecting in one of the public domains a new and magnificent observatory furnished with all the improvements and appliances of modern science. In order to effect this, it would suffice to alienate about 22,000 square metres of gardens and open spaces surrounding the present Observatory, and serving only to isolate it from the neighbouring houses. Sold at the moderate estimate of from 4*l.* to 6*l.* per metre, a sum of nearly 120,000*l.* might be raised, which would be more than sufficient for the purpose.

After sacrificing enough land for the construction of two new streets in continuation of the Avenue du Luxembourg, and isolating the Observatory on all sides, it would still retain the northern court and a garden on the south 70 to 80 metres long by 50 broad. The building would thus also retain the exact appearance that it presented when originally constructed by Perrault. Here might be preserved the Archives, the Bureau des Calculs, the Museum, and three or four instruments still capable of rendering some service if placed at the disposition of the Faculty of Sciences for the instruction of students.

All the plans of some such project as is here proposed have already been prepared with the greatest care by the able architect, M. Deharme. They include accommodation for thirty astronomers and assistants with their families, all the instrumental and service rooms, the halls, and an underground gallery, a structure 300 metres high for the study of the atmosphere, gas works, a covered gallery connecting all the instruments with the apartments of the astronomers; lastly, the great cupola for the 16m. telescope, at a total cost of 98,350*l.* Including the price of the new instruments, fittings, and inclosing wall, this sum would be raised to 108,000*l.*, which might be obtained by the proposed sale of lands.

The Council has unanimously adopted this project, demanding that it be referred to the Academy and to the Bureau of Longitudes, which bodies had already pronounced favourably on some such scheme in 1854 and 1868. Thus no serious objections seem to stand in the way of a project by which alone the present adverse conditions may be removed, and France endowed with the most complete and finest observatory of modern times.

## *UNIVERSITY AND EDUCATIONAL INTELLIGENCE*

OXFORD.—The Sherardian Botanical Chair at Oxford has at length been filled up by the election of Mr. Bayley Balfour, Professor of Botany at Glasgow. Mr. Balfour has had a distinguished career. Passing his student life at Edinburgh, he finally graduated as a Doctor of Medicine, receiving the University Gold Medal for his thesis, having previously carried off first class honours as Doctor of Science in Botany. Two years were spent in acquiring a practical knowledge of the methods of morphological and physiological research in the botanical laboratories of France and Germany under Profs. De Bary and Sachs. We next find him assisting his father, the Regius Professor of Medicine and Botany in the University of Edinburgh, in conducting his classes alike in the lecture-room, in the laboratory, in the herbarium, and in practical field work. For four years he was assistant to the Regius Professor of Natural History in the University of Edinburgh, and for six years he lectured on botany to the students of the Royal Veterinary College, until finally he was appointed Crown Professor of Botany in the University of Glasgow. Of good scientific work done there is an ample record. A valuable paper published in the *Philosophical Transactions* gives the result of his labours at Rodriguez, where he was sent by the Royal Society in 1874 as botanist and zoologist to the Transit of Venus Expedition. In 1880 we find him making a scientific exploration of the Island of Socotra, the results of which have been published in various periodicals, the final report on the botany of the island being now in course of publication by the Royal Society of Edinburgh. Prof. Balfour's wide experience in field, laboratory, and herbarium, will make him a valuable addition to the Natural

Science Staff of Professors in Oxford. As Magdalen College has under its new statutes added a Fellowship to the endowments of the Chair, we may congratulate the College on gaining another addition to its already long list of distinguished Natural Science Professors who are members of the Society. Profs. Westwood, Burdon Sanderson, Odling, Lawson, Balfour, Daubeny, Phillips, Brodie (now dead), were all members of the College.

On May 6 an examination will be held at New College to elect an Exhibitioner in Natural Science. The Exhibition will be given for proficiency in Chemistry and Biology.

At Magdalen College an open Demyship will be offered for Natural Science in June next.

THE University College (London) School "Old Boys" annual dinner will be held this year at the Holborn Restaurant, on Tuesday, February 19, at 7 p.m.; George Buchanan, M.D., F.R.S., in the chair.

THE Central Institution of the City and Guilds of London Institute in Exhibition Road is now approaching completion, and the Executive Committee are proceeding to appoint, in the first instance, four professors to the chairs of Chemistry, of Engineering, of Physics, and of Mechanics and Mathematics respectively. The salary attached to each professorship will be 1000*l.* per annum, with a prospect of increase depending upon the number of students in attendance. It is expected that the appointments will be made during the next few weeks. The Council of the Institute, at the request of the Duke of Buckingham and Chandos, have consented to lend, during the summer months, and pending the preparation of the fittings, a portion of the Central Institution to the Commissioners of the International Health Exhibition for the display of appliances for scientific and technical instruction and of the work done in technical schools here and abroad.

## *SCIENTIFIC SERIALS*

THE *Journal of Physiology*, vol. iv. Nos. 4 and 5, December, 1883, contains:—An account of the discussion which took place in the Physiological Section of the International Medical Congress held in London, 1881, on the localisation of function in the cortex cerebri. Prof. Goltz of Strasburg, it will be remembered, exhibited a dog, and Profs. Ferrier and Yeo a monkey. The brains of these animals were handed over to a Committee, consisting of Dr. Klein, Mr. Langley, and Prof. Schäfer. The report of this Committee is preceded by a memoir on the normal structure of the dog's brain, by J. N. Langley (plates 7 and 8), and the report consists of a report on the parts destroyed on the right side of the brain of the dog operated on by Prof. Goltz, by J. N. Langley (plates 9 and 10); of a report on the parts destroyed on the left side of the brain of the same dog, by E. Klein (plate 11); and of a report on the lesions primary and secondary in the brain and spinal cord of the Macaque monkey exhibited by Profs. Ferrier and Yeo, by E. A. Schäfer (plate 12).

THE *Journal of the Royal Microscopical Society* for December, 1883, contains:—On some new Cladocera of the English lakes, by Conrad Beck (plates 11 and 12).—On an improved method of preparing embryological and other delicate organisms for microscopical examination, by Edward Lovett.—On the relation of aperture and power in the microscope, by Prof. E. Abbe.—On a new camera lucida, by Dr. Hugo Schröder.—On optical tube length, an unconsidered element in the theory of the microscope, by Frank Crisp.—Also the usual summary of current researches relating to zoology, botany, and microscopy.

THE *American Naturalist* for December, 1883, contains:—On the development of a dandelion flower, by John M. Coulter.—Notes on *Chætonotus larus*, by C. A. Fernald.—Notes on the aborigines of Cooper's Creek, Australia, by E. B. Sanger.—Zoological gardens, a critical essay by Theodore Link.—The Copperhead, by Dr. R. E. Kunze.—Experiments with the antennæ of insects, by C. J. A. Porter.—On the position of the Compositæ and Orchideæ in the natural system, by J. F. James.—On the habits of certain sunfish, by C. O. Abbott.—Recent literature, and general notes.

*Revue Internationale des Sciences Biologiques*, October 15, 1883, contains:—Translations of Mr. W. S. Duncan's—Probable region of man's evolution, and of Prof. Huxley's—Living beings and the method of studying them; Dr. Hubrecht—on the ancestral form of the Chordata; and Dr. W. G. Parker—on the people and language of Madagascar.

The number for November 15, 1883, contains:—An essay by Dr. Lanessan, on Buffon: his ideas, his *rôle* in the history of science, his work, and on the development of the natural sciences since his epoch, which essay is to serve as an introduction to a complete edition of Buffon's works, including his correspondence, to be shortly published by Le Vasseur, Paris.

*Rendiconti del Reale Istituto Lombardo, Milan*, December 29, 1883.—Reports on the work of the various physical, literary, ethical, mathematical, and political sections of the Institute during the year 1883, by the Secretary.—Meteorological observations made at the Brera Observatory, Milan, during the month of December, 1883.

*Nachrichten von der K. Gesellschaft der Wissenschaften und der Universität zu Göttingen*, December 1, 1883.—On the formation of isomerous derivatives of toluol, by Paul Jannasch.—On the irreducibility of linear differential equations, by Leo Königsberger.—On the polar repulsion, the coefficient of induction, and temperature of a magnet, and on the determination of the moments of inertia through bifilar suspension, by F. Kohlrausch.

## *SOCIETIES AND ACADEMIES*

### LONDON

**Royal Society**, January 10.—"Extracts from a Report on the Volcanic Eruption in Sunda Strait by Commander the Hon. F. C. P. Vereker, H.M.S. *Magpie*, dated Singapore, October 22, 1883." Communicated by Sir Frederick Evans, K.C.B., F.R.S.

. . . On the 18th inst. I entered Sunda Strait, passing east of Thwart-way Island. This island had been reported to be split by the eruption into several portions. This is incorrect.

The island is intersected by low valleys in several places; these being covered with tall trees did not show so prominently formerly as they do now. The whole of the vegetation having been swept away by the tidal wave, the island at a short distance off is apparently divided, the low necks joining the higher portions being only visible on close approach.

The surface of the Strait in this neighbourhood is covered with extensive fields of floating pumice-stone, often in one to two foot cubes, through which the ship easily forced her way. . . .

I inclose sketches which I trust will convey the general appearance better than a written description. The whole of the neighbourhood is covered with greenish yellow mud, and all traces of vegetation everywhere destroyed.

I communicated personally with the captain of the Netherlands frigate *Queen Emma* stationed on the spot, and was informed by him that the changes are considerably more extensive than was at first thought, and that Verlaten Island is still in a state of activity as well as Krakatoa itself.

From observation he thinks that another eruption is impending, but that Verlaten Island will be the centre of disturbance.

The Netherlands Government vessel *Hydrograaf* obtained a sounding of 100 fathoms without reaching bottom, in the centre of the group and off the cliff falling from Krakatoa Peak.

The two new islands are low mud and pumice banks, their configuration is continually altering, and I was informed that they are gradually subsiding. . . .

It is still impossible to examine Lampong Bay, but the pumice-stone is now beginning to float out.

The light on Fourth Point (Java) has been temporarily replaced by one of the 6th order, visible five miles, but beside this there are no signs of life on the Java shore. The whole coast is covered with the debris of trees, &c., demolished by the earthquake sea-wave, and over all lies a thick incrustation of volcanic mud.

During the height of the eruption a terrific whirlwind and a fierce south-westerly gale, apparently local, was experienced. . . .

**Victoria Institute**, February 4.—Mr. Ernest Budge, B.A., of the Oriental Department of the British Museum, read a paper upon a new and important inscription of Nebuchadnezzar the Great. Two copies of the same text had been brought to England by Mr. Rassam, one of which was much mutilated, but by a careful comparison of the texts Mr. Budge has succeeded in gaining a nearly perfect copy of the inscription. It related chiefly to the restoration of the fortifications of Babylon—the great walls, gates, and quays along the river bank, which had been thrown down by the conquering armies of Sowgon, Sennacherib, and Assurbanipal. It also stated the area of the citadel of Babylon

was 4000 square cubits. The inscriptions described the restoration of the famous temple of Belus, which was made "bright as the beauty of heaven," with gold, silver, crystal, and precious stones; the roof of the "house of the oracle" was of cedar wood, plated with gold. The King recorded the restoration of many other public and sacred edifices, and among others the Tower of Borsippa, known as the Tower of Babel, according to Babylonian tradition. In concluding the inscription, the King, in a most beautiful prayer, commended his pious works to the keeping of "Merodach, King of Heaven and Earth," to whom he prayed "for long life, fullness of glory, and a widespread dominion."

EDINBURGH

**Mathematical Society**, February 8.—Mr. A. J. G. Barclay, vice-president, in the chair.—A presidential address was delivered by Mr. Thomas Muir, F.R.S.E., on the promotion of research. Attention was drawn to the backward state of mathematical research in Scotland, particularly when compared with the activity of Germany in the same department. Some of the causes of this were discussed, and methods were suggested for bringing about a reform.—Mr. H. H. Browning, Glasgow, contributed a paper on illustrations of harmonic section; and Mr. Muir communicated a theorem regarding the area of a polygon of $2n$ sides.

PARIS

**Academy of Sciences**, February 4.—M. Rolland in the chair.—Note on the necessity of establishing a branch of the Observatory outside of Paris, by Admiral Mouchez.—On a new application of the mercurial level suggested by M. Renouf for calculating the altitude of the stars at sea when the horizon is invisible, by Admiral Mouchez. This ingenious contrivance, which is available on land as well as on sea, almost completely removes the difficulties hitherto experienced in obtaining altitudes within 4′ or 5′ at night or in foggy weather. The apparatus, made by M. Hurlimann, mechanician, has been for some time in use on board the Transatlantic steamers plying between France and the United States. M. Mouchez describes it as much simpler and more exact than any other system hitherto invented.—On an optical phenomenon observed during a fire that broke out at Joly on January 31, by M. E. Chevreul. For three-quarters of an hour the light of the street gas presented the complementary colours of the light of the fire, that is, from yellow-green to green and bluish, the sensations being referable at once both to the simultaneous and successive contrast, according as the observer beheld both lights simultaneously, or one only at a time.—On Faraday's law regarding an electric current traversing a series of electrolisable salts during the same time, by M. Berthelot. The author argues that Faraday's law is in general more simply expressed by means of the equivalents than by the atomic weights, both for the electro-positive and for the electro-negative elements.—Reply to M. Richet's remarks on the method of anæsthesis by means of the titrate mixtures of chloroform and air, by M. Paul Bert.—Curves registered by the mareograph established at Colon (earthquakes at Santander, Guayaquil, Chios, &c.), by M. de Lesseps. The curves recorded on October 13 and 14, 1883, appear to have indicated the underground disturbances caused by the earthquakes that occurred on those dates at Santander on the Atlantic, Guayaquil on the Pacific, Chios in the Mediterranean, and elsewhere. Yet nothing abnormal was registered by the mareograph of the island of Naos, Gulf of Panama.—On the quantities forming a group of nonions analogous to the quaternions of Hamilton, by M. J. J. Sylvester.—*Résumé* of the meteorological observations made during the year 1883 at four points in the Upper Rhine and Vosges districts (Colmar, Munster, Schlucht, and Thann), by M. G. A. Hirn. Referring to the recent twilight effects observed at these stations, the author feels justified in concluding that the particles, whether gaseous or in the form of dust, lit up by the solar rays, were situated, at least to a large extent, beyond the terrestrial atmosphere, in any case at elevations where no traces have ever been observed either of cirrus or vapour of water.—On the late twilight phenomena, by M. de Gasparin. The author considers that the chief features of these phenomena were their rapid appearance from fifteen to sixteen minutes after sunset, and their constant recurrence in a given place for a period of sixty-six days.—On an instrument capable of producing in the same telescope the images of two stars at the moment when they are at the same altitude, and of furthe determining by a single observation the astronomic time o the place, its latitude, and exact position for the whole horizon, by M. Ch. Rouget.—On biquadratic involutions, by M. C. Le Paige.—On a class of abelian functions and on a hyperfuchsian group, by M. E. Picard.—Note on the exact number of variations obtained in the multiplication of the integral polynome $f(x)$ by the binome $x + a$, by M. D. André.—Transelementation of glyoxal into glycolic acid, by M. de Forcrand.—On the thermal properties of the numerous oxichlorides of mercury, by M. G. André.—Researches on the formation of the crystallised fluoride of antimony and its dissolution either in pure water or in solutions of fluorhydric acid, by M. Guntz.—On the heat of transformation of the prismatic oxide of antimony into octahedric oxide, by M. Guntz.—On the liquefaction of hydrogen, by M. S. Wroblewski. From the results already obtained, the author supposes that the temperature required for the complete liquefaction of hydrogen is about that which may be obtained by means of boiling oxygen.—On a case of isomerism of chloronitrous camphor, by M. P. Cazeneuve.—On the segmentary organs and the podocyst of the embryos of the slug family, by M. S. Jourdain.—On the Tongrian deposits at Longjumeau, Department of Seine-et-Oise, by M. Stan. Meunier.—On some freshwater formations of the Tertiary period in Algeria, by M. Ph. Thomas.—On the influence of oxygen under increased pressure on the cultivation of *Bacillus anthracis*, by M. J. Wosnessenski.—On the cause of the twilight effects of 1883, by M. G. Tissandier. Accepting M. Angot's assumption that hypotheses inapplicable to the year 1831 must be rejected for 1883, the author shows that the atmospheric conditions of both years resembled each other in every respect. The circumstances attending the eruptions in the Sicilian waters in 1831, when the volcanic island of Pantellaria made its appearance, were completely analogous to those of the Krakatoa eruption in 1883. On both occasions the optical phenomena were immediately preceded by igneous disturbances ejecting into the atmosphere vast quantities of gaseous products and fine dust. Hence the probability that to volcanic eruptions were due the optical manifestations in both years.—On the twilight effects of the last few months, by M. Perrotin. This author also argues that the twilights of 1831 prove nothing against, but rather confirm, the volcanic theory adduced to account for those of 1883.

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